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THE SIGN AND NAME FOR PLANET IN BABYLONIAN.

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Kugler begins his valuable work on Babylonian astronomy[1] with a discussion of the ordinary name for planet in Babylonian, namely, *bibbu*, and for which the ideographic designation is LU-BAT.[2] He

[1] "Sternkunde und Sterndienst in Babel" (Münster, 1907), I., pp. 7–9.

[2] That this combination is used for planet in general follows from such passages as (1) Thompson, "Reports of the Magicians and Astrologers," No. 112 Rev. 7; 236 B Rev. 4, where LU-BAT occurs with the plural sign to designate the planets in general; See also nos. 88 Obv. 4 and Rev. 1; 89 Rev. 6; 101 Obv. 5; 103 Obv. 6, Rev. 7; 163 Obv. 4; 167 Rev. 1; 172 Rev. 1 and 3; 175 Obv. 4; 200 Rev. 5; 216 Rev. 1; 218 Obv. 1; 218 A Obv. 5; 219 Obv. 1; 220 Obv. 1; 222 Obv. 1; 223 Obv. 1; 224 Obv. 3; 225 Obv. 4; 229 A Obv. 1, 2, 4; 232 Rev. 1; 234 Obv. 3; 234 A Rev. 1; 235 Obv. 11; 244 C Obv. 6, where LU-BAT is used for planet in general. It is to be noted, however, that the only planets which are regularly designated by means of LU-BAT are Mercury (LU-BAT GU-UD) and Saturn (LU-BAT SAG-UŠ). So in the famous list of planets IIR 48; 50–54 a-b and IIIR 57, No. 6, 65–67, and Thompson, *l. c., passim*, though occasionally even in the case of these two planets the element LU-BAT is omitted, *e. g.*, Thompson, *l. c.*, Nos. 105 Obv. 8; 215 Obv. 1; 217 Obv. 1; 223 Obv. 4; 228 Obv. 1; (GU-UD) and 167 Rev. 4 (SAG-UŠ). Further references in Kugler, *l. c.*, p. 12. Occasionally also Mars is designated as LU-BAT DIR instead of (il) ZAL-BAT-(a-nu), so *e. g.*, Thompson No. 146 Rev. 4–6, and 195 Rev. 1–2, where in both cases a gloss LU-BAT DIR = (il) ZAL-BAT (-a-nu) furnishes the proof for the identification. In the later period (after c. 400 B. C.) Saturn is designated as GI and Mars as AN. See Kugler *l. c.*, p. 12, including the note on that page.

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accepts the interpretation proposed as long ago as 1890 by Jensen[3] for the ideographic compound as "frei weidendes, abseits weidendes Schaf." This view rests on the identification of the first sign LU as "sheep," while the second is taken in the sense of "to remove,"[4] the combination thus expressing the movement of the planets, like sheep that wander away from the flock. That Jensen was right in his explanation of the first element as "sheep" follows from various considerations, among which the testimony of the lexicographical list IIR. 6, 4 c–d by itself, LU-BAT = *bi-ib-bu*, is decisive, since in the same list LU-IGI is explained as *lu-li-mu* (l. 8) "ram" or "bell-wether" and LU is the common ideogram for *immeru* the ordinary term for "sheep."[5] In addition we have the equation IIR. 39, No. 5, 62 a–b (*il*) *bi-ib-bu* = (*il*) LU-BAT.

Jensen's explanation, however, of the second element is not satisfactory. In the first place the equation BAT = *nisû* ("to remove") does not represent the most common value of the ideograph in question, for the various meanings of which it seems more reasonable to start from the fundamental notion of "coming to completion,"[6] whence we have the further development in two directions: (1) "coming to an end" (gamâru, katû, Br. Nos. 1499, 1512, etc.), "closing up" (sakku, sikêru Meissner, Assyr. Ideogramme, Nos. 869–872); "removing" (nisû, Br. No. 1525); "growing old" (labâru, Br. No. 1515); "die" (mâtu, etc., Br. Nos. 1517–19, 1527, 1533); "set at rest" (pašâhu, Br. No. 1528): (2) "Completion" in the sense of "fullness" and "vitality," consequently, "life" (balâtu, Br. No. 1494); "being" (bašû, Br. No. 1495); "blood"

[3] Kosmologie der Babylonier, p. 99. Hommel (Aufsätze und Abhandlungen p. 379) thinks that the designation *bibbu* which he takes as "ram" is an allusion to the "solar" character of the planets, but this is even less plausible than Jensen's explanation.

[4] Cf. Brünnow, No. 1525 (BAT = nisû).

[5] See Muss-Arnolt, Assyr. Dict., p. 61 b. Note also that in the list IIR. 6, 5–8 we have the group bi-ib-bu, a-tu-du ("he-goat"), šap-pa-ru ("mountain-goat") and lu-li-mu.

[6] We must bear in mind as Thureau-Dangin, "Recherches sur l' Origine de l'Ecriture cuneiforme," No. 11, has pointed out, that two originally distinct signs have been confounded in BAT, so that all meanings associated, *e. g.*, with *pitû* (Br. No. 1529) must be referred to No. 278 (p. 45) and explained accordingly.

(dâmu, Br. No. 1503), and "rule" (bêlu, etc., Br. No. 1496; Meissner, No. 856), as manifestations of vitality and power as well as "strong" (ikdu, Meissner, No. 851), "protect" (emû, Meissner, No. 853), etc. The idea of "removing" falls, therefore, in the category of a secondary or tertiary derivative from the fundamental value of the sign BAT. In the second place, it is rather a violent transition from the sense of "removing" to that of "pasturing by itself" and the like. Nor does the metaphor introduced in the Babylonian creation epic[7] (Tablet VII., 111, ed. King) where the stars, or rather the gods, are compared to sheep under the guidance of Marduk strengthen the conclusion that the planets are sheep that "pasture aside" from the stars in general, since the passage does not refer specifically to the planets. This passage, as well as the others adduced by Kugler (*l. c.*, p. 7), merely justifies the interpretation of the first element in LU-BAT as "sheep." For the second element we must start from the much more common meaning attaching to the sign in question, namely, "dead" (*mîtu*). The Babylonians themselves had this equation in mind when they explained LU-BAT as *muš-mit bu-lim*, "causing cattle to die" (VR, 46, No. 1, rev. 41) even though this explanation is to be regarded as a fanciful one.[8]

Taking the two signs as they stand, the simplest explanation is to interpret them as "dead sheep" in the sense of a sacrificial animal. To the question which now arises, what connection is there between the planets and "dead sheep," the divination texts, I venture to think, furnish a satisfactory answer.

II.

On the basis of recent researches,[9] we must distinguish in Baby-

[7] See Kugler, *l. c.*, p. 7.

[8] Recognized as fanciful by Jensen, Kosmologie, p. 96. Kugler's attempt (*l. c.*) to reconcile this explanation with the interpretation offered in astrological texts whereby certain phenomena connected with the planets prognosticate death is very artificial and encounters a fatal objection from the consideration that the prognostication of death in one form or other, is a common interpretation of omens, indeed one of the commonest. See examples in Jastrow, "Religion Babyloniens und Assyriens," II,. pp. 261, 298, 328, 329, 331, 333, 343, etc.

[9] See Jastrow, *l. c.*, pp. 212 f., and various papers by the writer as, *e. g.*, "Signs and Names of the Liver in Babylonian" (Zeitschrift für Assyr. XX., p. 111 f.), "The Liver in Divination and the Beginnings of Anatomy" (University of Pennsylvania Medical Bulletin, January, 1908).

lonian-Assyrian methods of divining the future two classes: (1) what we may call voluntary divination, and (2) involuntary divination. The characteristic feature of voluntary divination lies in deliberately seeking out some object by means of which an answer to a specific question regarding the future or the outcome of an undertaking, a sickness or what-not is furnished. The signs furnished by the liver of an animal selected as a sacrifice belong to this category; likewise the observation of the flight of birds sent out for the purpose of securing omens, the throwing of arrows before the image of a deity and the like. Involuntary divination, on the other hand, is concerned with the attempt to interpret signs that *force* themselves on our notice, such as phenomena connected with the sun, moon, planets and stars, the movements of clouds, earthquakes and storms; the actions of animals—dogs, snakes, locusts, birds, etc., that one happens to encounter and all the unusual or significant happenings and accidents in human life, while dreams form a special subdivision in this class of involuntary divination. We might for the sake of convenience distinguish the signs furnished by voluntary divination as "omens" and those of involuntary divination as "portents," but however we may distinguish them, the recognition of these two distinct classes is fundamental to an understanding of the general subject of divination.

Confining ourselves to Babylonia and Assyria, the chief method of voluntary divination was the inspection of the liver of the sacrificial animal and the chief method of involuntary divination, the observation of phenomena of the heavens. The correctness of this thesis is shown by the wide scope of these methods as revealed in the texts themselves.[10] Both methods rest on a well-defined theory, the inspection of the liver on the basis of the primitive view that the liver was the seat of vitality, of the intellect, of both the higher and lower emotions—in short, the seat of the soul, as that term was popularly understood.[11] The deity in accepting the sacrificial animal identifies himself, as it were, with the animal, becomes

[10] See Jastrow, *l. c.*, II., p. 209 f.—especially note 1 on p. 210. See parts 11–12 of this work for "liver" omens and the forthcoming parts 13 and 14 for "astrological" omens.

[11] Jastrow, *l. c.*, pp. 213 f.

one with it and, accordingly, the liver of the animal reflects the mind and will of the god. If one can therefore read the liver correctly, one enters, as it were, into the workshop of the deity. The mind of the animal and the mind of the deity become for this specific occasion like two watches regulated to be in perfect accord.

The divining of the future through the observation of the phenomena in the heavens rests on the identification (or personification) of the gods with the sun, moon, planets and stars. The movements of these bodies, the changes in their aspects and the variations in their relationship to one another represent, as it were, the activity of the gods and since, according to the current theory, all happenings on earth are due to the gods or to one god or the other, a knowledge of what the activity in the heavens portends furnishes the means of foretelling what is to happen on earth. In time no doubt the theory was perfected, at least in the theological circles of Babylonia and Assyria, into a complete correspondence between occurrences on earth and the decision to bring about these occurrences by the manifested activity of the gods in the heavens; but even without the perfected theory, the repeated observation of the kind of happenings on earth coincident with conditions and phenomena in the heavens would have led to attaching importance to these conditions—both those of a usual order and those of a more or less unusual nature.

Of these two chief divisions of divination, it is evident that the inspection of the liver, connected as it is with a primitive view of that organ, can be accounted for as the distinct outgrowth of popular beliefs, whereas the divination through the phenomena of the heavens not only makes greater demands on scientific or pseudo-scientific knowledge but presupposes also a conception of world-philosophy which can hardly be termed popular. The personification of the sun and moon is, of course, an element in all primitive phases of belief, but the extension of such personification to the planets and stars belongs to a higher order of thought, since the bearings of those bodies on the life, happiness and fate of mankind are of a more remote and a more indirect character than in the case of the two luminaries; and when we come to the projection of practically all the activity of the gods on to the heavens, we have definitely passed beyond the intellectual range of popular fancy and

have entered the domain of distinctly theological speculation. If the views of the school associated with the names of Winckler and Jeremias, that the entire Babylonian religion is under the sway of "astral" conceptions, turn out to be correct, it will also have to be recognized that the underlying "Weltanschauung" is a product of the schools rather than an expression of popular notions.[11a] I venture to think that one of the weaknesses of the "astral" theory, which has from other points of view so much in its favor, is this failure on the part of its promoters to recognize the essentially "learned" character of what according to them became the prevailing world-philosophy in the ancient Orient and which must for a long time at least have separated it sharply from the much lower plane of popular beliefs and fancies.

Be this as it may, the development of a method of divination, through elaborate observations of the movements and positions of sun, moon, planets and stars, it will be admitted, belongs to a later stage in the unfolding of religious rites than so primitive a method as the inspection of the liver of a sacrificial animal. The persistence of astrology among advanced cultures as in India and Persia and in western Europe[12] down to the threshold of modern times, whereas "liver" divination disappeared with advancing culture everywhere except among the Babylonians and Assyrians and the Greeks, Romans and Etruscans,[12a] clinches the argument in favor of divination through the liver as the earlier and more primitive method. If this be admitted, it would be reasonable to find in the

[11a] See also Comont, "Les Religions Orientales dans le Paganisme Romain" (Paris, 1907), p. 197.

[12] See the summary by Jeremias, "Das Alte Testament im Lichte des Orients" (1 ed.), p. 7, note 1.

[12a] Roman divination is dependent upon Etruscan, while in the case of Greek divination it is still a question whether we are to assume direct influence from Babylonia or likewise through the mediation of the Etruscans. In either case we have only two systems of "liver" divination surviving among cultured nations—the Babylonian and the Etruscan; and further investigations may definitely confirm the view which on the surface seems plausible that "liver" divination among the Etruscans stands in some direct connection with Babylonian divination. If this be so, then the single cause to which the persistence of "liver" divination in certain quarters is to be ascribed is the elaboration of the complicated and ingenious system of interpretation which we owe to Babylonian priests. See Jastrow, II., pp. 215 and 320, note 3.

later method of divination through the heavens, traces of the earlier one, if not indeed some link directly connecting the two. Among the Etruscans we actually encounter such a link in the interesting circumstance that the famous "bronze liver" of Piacenza,[13] prepared like the Babylonian clay model of a liver[13a] as an object lesson for instruction in the temple schools, is divided off along the margin into sixteen regions, corresponding with the ordinary divisions of the heavens and that the forty Etruscan words with which the surface of the liver is covered are names of deities. Whether we accept Thulin's view,[14] who sees a direct relationship between the enumeration of the gods and the list and arrangement given by Martianus Capella, or follow Körte,[15] in either case the "liver" reproduces the recognized divisions of the heavens and through this combination the liver becomes, as it were, a microcosm reflecting the macrocosm. The much-discussed problem[15a] whether this remarkable object dating from about the third century B. C. is a "liver" or, as was first supposed, a "templum," thus resolves itself into the thesis that it is both. To use the words of Körte in his paper in summarizing the results of twenty-five years of study of this object:[16]

> "The liver as the seat of life according to the view of antiquity appears as a minature reproduction of the universe. As the latter, so the liver is divided into a right and left half,[17] a day division and a night division, the line of division corresponding to the line dividing the universe into east and west. As the vault of heaven, so the edge of the liver is divided into 16 regions in which the gods who furnish portents dwell."

[13] See Körte, "Die Bronzeleber von Piacenza" (*Mitteil. d Kaiserl. Deutsch. Archæolog.-Instituts.*, XX., pp. 348–379), the latest and probably final word on the subject.

[13a] Cuneiform Texts, VI., Pl. 1–2 and photograph.

[14] "Die Götter des Martianus Capella und der Bronzeleber von Piacenza" (Giessen, 1906), pp. 31–59.

[15] Körte, *l. c.*, p. 367 f.

[15a] See the references in Körte, p. 349 f., to which Nicola Terzaghi, "La piu recente Interpretazione dei Mundus-Templum di Piacenza" (*Bolletino Storico Piacentino*, 1906, Maggio-Giugno) is to be added.

[16] Körte, *l. c.*, p. 362.

[17] Referring to the band on the reverse of the object. See the illustration in Körte's article, p. 356.

III.

If, therefore, among the Etruscans we find the unmistakable proof of a direct link between the two classes of divination, we should be prepared to find a similar association in Babylonia and Assyria. I believe that the ordinary name and sign for planet in Babylonia points in this direction. While already in early days we find various animals and all kinds of products dedicated as offerings to the gods,[18] for purposes of divination the only animal set aside was the sheep. This follows not only from the fact that the famous clay model of a liver found near Bagdad is that of a sheep,[19] but from the specific references to sheep in "liver" divination texts *and to no other animal.*[20] The sheep thus becomes the animal of divination *par excellence,* and we can well suppose that the word itself should come to be used as synonymous with divination. Such a usage would be paralleled by the extension of the term *auspicium* in Latin, which from being an omen derived through "bird observation" was applied to any kind of an omen or portent, so that an inspection

[18] See Thureau-Dangin, "Die Sumerischen und Akkadischen Königsinschriften" (Leipzig, 1907), pp. 16, 80, 84, 86, 88, 124, etc. I cannot here enter into a full discussion of the nature of sacrifices among the Babylonians and Assyrians but it may be proper to point out that in an elaborate ritual controlled by an extended priestly organization we must sharply differentiate between (1) offerings that constitute part of the income of the temples, (2) voluntary gifts, (3) sacrifices offered in connection with purification or expiatory rites and (4) sacrifices offered directly to and for the god. So far as I can see sacrifices of the latter kind were brought only when an answer to a specific question was desired, so that it would appear that divination forms the starting point for the development of the whole idea of sacrifice in the proper sense in Babylonia.

[19] CT, VI., Pl. 1. See Jastrow, *l. c.,* II., p. 218 note 1, where a reference should have been given to Stieda, "Ueber die aeltesten bildlichen Darstellungen der Leber" (Bonnet-Merkel, Anatomische Hefte XV., p. 697), who shows that it is (as also in the case of the bronze liver of Piacenza) the liver of a sheep and not of a goat—as had been supposed by some scholars.

[20] *E. g.,* CT, XX., 1, 1—in the opening line of the first tablet of a series dealing with "liver" divination; also Boissier, "Documents Assyriens relatifs aux Prèsages," p. 97, 11; 212, 27; also in the "omen" text CT., IV., Pl. 34, 9; in the omen report of the Cassite period (Clay, *Cassite Archives,* XIV., Pl. 4, Obv. 10, and lastly the constant mention of the "sheep" in the omens attached to Knudtzon, *Assyr. Gebete an den Sonnengott.* Note also the expression *bêl immeri* "owner of the sheep" (CT, XX., 33, 93 and Boissier, Documents, p. 96, 13). The addition of NITA to LU shows that a "male" sheep was selected for the purpose.

of the liver of an animal for the purpose of securing an "omen" was also designated as an *auspicium*.[21] Similiarly, in Greek the word ὄρνις, "bird," is used for any kind of an omen and my colleague, Professor Lamberton, has kindly called my attention to the interesting passage in the *Birds of Aristophanes* in which this usage finds a striking illustration. In the "Parabasis," after indicating all the blessings that accrue to men from the birds, the chorus turns to divination and continues as follows:[22]

"You consider all things a bird, whatever gives a decision through divination. With you a word is a 'bird,' and you call a sneeze a 'bird,' a sound a 'bird,' a sudden meeting a 'bird,' and an ass a 'bird.' Are we not clearly a prophetic Apollo to you?"

The sheep, being the animal of divination *par excellence* in Babylonia, would in the same way become the Babylonian term for an "auspicium" in general. If we assume that this use of the term lurks in the application of "sheep" as the designation of a planet, a satisfactory explanation can be found for the addition of the sign BAT to the sign for "sheep" which has more specifically the same force in the combination LU-BAT as in the combination IIR. 27, No. 2, Obv. 46, c–d, UR-BAT, *i. e.*, "dead liver" in the sense of the liver of a sacrificial lamb and hence as the equivalent of *ter-tu ša ha-še-e*, "omen through the liver."[23]

The combination LU-BAT thus expresses more precisely than LU alone the association of an "omen" with a "sheep," and we would be justified in rendering the combination as "sheep omen," and then through the association of ideas above pointed out, as a general term for "omen."[24]

[21] See Pauly-Wissowa, "Real-Encyclopaedie," (new ed.), II., p. 2580 f.

[22] Ll. 719–22 (ed. Van Leeuwen, Leiden, 1893). Dr. R. G. Kent, of the University of Penna., also calls my attention to the interesting passage in Xenophon's Anabasis (III, 2, 9) where a "sneeze" as a good sign is spoken of as ὀιωνός or "bird" in the general sense of an omen.

[23] On the word *ḫašû* for liver which may have been used in earlier days in place of *kabittu* see Jastrow, *l. c.*, II., p. 213, note 1, and p. 276, note 7.

[24] It is to be noted that at least in one passage in a "liver divination" the sign BAT is added to LU-NITA "male sheep," namely Boissier, *Doc. Assyr.*, p. 212, 27, ultu libbi LU-NITA BAT (u) têrtu (written UR-BAT as in the passage IIR 27) tu-še-la-a, *i. e.*, "Out of a dead sheep thou shalt bring forth an omen," where the phonetic complement *u* added to BAT suggests the reading *mîtu* and where "dead sheep" is clearly the equivalent of "sacrificial sheep" or "omen sheep."

Now what was the purpose for which the movements of the planets were observed by the Babylonians? What other than to secure through such observation, signs by means of which the future could be divined? The planets were, primarily, regarded as "omens" and since, as has been above set forth, divination through the heavens follows in point of time divination through the liver of the sheep, we would expect conceptions and terms used in "liver" divination to be transferred to astrological divination. The use of the term "sheep" as the designation of the planets observed to secure omens, precisely as omens were furnished by means of sacrificial sheep, I, accordingly, take as an illustration of this dependence of astrology upon hepatoscopy, forming, as it were, the connecting link between the two. It may be noted in this connection that the interpretations given in astrological texts to signs observed are paralleled in the "liver" divination texts,[25] and there can be little doubt that they are transferred bodily from the latter and earlier class of texts to the former.

The explanation here proposed, according to which LU-BAT as applied to the planets conveys the notion that they were regarded as "omens" or means of securing omens, throws a new light upon the statement in Diodorus[26] that the Babylonians commonly called the five planets ἑρμηνεῖς, *i. e.*, "interpreters," adding as a reason for the designation that the planets were regarded as "interpreting" for mankind the intention of the gods. Bouchè-Leclercq ("L'Astrologie Grecque," p. 40, note 3), recognizes that the term "interpreters" does not embody a Greek tradition, but the notice in Diodorus, so far from being, as he supposes, of "doubtful value," reflects the perfectly correct view that the planets were used as "omens"[27] and the term "interpreters" is evidently an attempt to

[25] The interpretations in the "astrological" texts are in fact practically identical with those in "liver" divination, furnishing the same references to public events and differing merely in containing more references to crops, to prices of food and to famine. Cf., *e. g.*, Craig, "Astrolog.-Astronom. texts," Pl. 2, 3; 20, 22 with CT, XX., 26, Obv. 3; Boissier, Doc. Assyr., 7, 21; Craig 20, 31 with CT, XX., 32, 54; 99, 100 (where ilu = Nergal). Cf. Jastrow, *l. c.*, II., p. 342, note 11).

[26] Bibl. Histor., Book II. (ed. Dindorf), 30, 4.

[27] To be sure, what Diodorus says in addition why the planets and not also the other stars were regarded as "interpreters" is rather beside the

convey this idea. The term may, therefore, be regarded as a rendering of the Babylonian designation "sheep omen" in the general sense above pointed out.

The objection may be raised at this point, why should not the moon and sun, as playing an equally if not more important rôle in divination lore, likewise have been designated as LU-BAT in the generic sense of an "omen" or "auspicium"? The answer is obvious. Sun and moon cults are such ingredient parts of early forms of religion everywhere and the dependence of human fortune, life, health and welfare upon these two luminaries is so direct that other factors were at work in the development of conceptions regarding these two deities than merely the observation of their movements and changing relationship to one another as a basis for determining what these deities were preparing for mankind. Their cult precedes their introduction into divination texts, whereas the planets were observed solely for purposes of divination. Since the influence of the latter on human life was a matter of speculation rather than of direct experience, the basic and primary motive for noting their movements was in connection with the view that, as representing gods, their movements indicated the activity of these gods in preparing the events that were to happen on earth. The old and long established names and designations for sun and moon were accordingly retained, whereas the new term chosen for the planets was ordinarily restricted to them. Occasionally, however, so, *e. g.*, III. R, 57, No. 6, 65–67, sụn, moon and the five planets are summarized as seven LU-BAT (pl.).

That the association of ideas did not, on the other hand, lead to the extension of LU-BAT to the stars in general constitutes no valid objection to the thesis here propounded. In the divination texts the number of stars introduced, outside of the planets, is not large and their role is quite secondary,[28] and it is not until we reach the period when astronomy becomes more definitely differentiated as a science from astrology, when calculations are made and "planet" tables are

mark; and shows that he no longer fully understood the force of the Babylonian designation which he here faithfully reproduces.

[28] In astrological texts proper as distinguished from astronomical tablets, the stars mentioned are chiefly certain ones belonging to the constellations of the ecliptic and which are frequently introduced as guides and indications for fixing the position of the planets, rather than as omens.

prepared independently of divination, that star-lore assumes larger dimensions. Besides, in securing omens the positions of the stars constitute a minor factor and are of value chiefly, if not exclusively, in relationship to phenomena connected with the planets—a condition which is specially applicable to the relationship between the planets and the constellations of the zodiac.

Attention has already been called to the fact that although LU-BAT is commonly applied to any planet, there are only two planets—Mercury and Saturn—that regularly appear written with this compound ideograph,[29] the former being designated as LU-BAT GU-UD,[30] the latter as LU-BAT SAG-UŠ, while Mars occasionally appears as LU-BAT DIR.[31] The other planets appear in the lists IIR, 48, 48–54 ab and IIIR, 57, No. 6, 65–67, as (il) ŠUL-PA-UD-DU-A (Jupiter) (il) DIL-BAT (Venus) and ZAL-BAT-a-nu (Mars), with MUL = *kakkabu* interchanging with AN = *ilu*. Moreover, the phonetic reading bi-ib-bu in the latter list for LU-BAT GU-UD points to Mercury as being the planet *par excellence*. Why should Mercury have been assigned to this preëminent position among the planets? It has been suggested to me[32] that the position of Mercury nearest to the sun may have led to its being looked upon as the chief planet for purposes of divination and it is perhaps not without significance that in Greek astrology Mercury, frequently designated as στίλβων, "shining,"[33] is closely associated with the sun, and indeed at times identified with Apollo.[34] Certainly, the peculiar conceptions connected with Mercury in the astrology of the Greeks and of other nations—whose dependence upon Babylonian beliefs and speculations is generally admitted—sharply separate that planet from his fellows. While the others, *e. g.*, are conceived as masculine or feminine, Mercury, and Mercury alone, is double sexed.[35] Qualities are heaped upon Mercury in profusion,

[29] See above, p. 141, note 2.

[30] Generally read GUD-UD but the reading GU-UD seems preferable.

[31] See above, p. 141, note 2.

[32] By my friend, Mr. H. H. Furness, Jr., whose suggestion commended itself to my colleague, Professor C. L. Doolittle, Director of the Flower Observatory (University of Pennsylvania).

[33] Bouché-Leclercq, "L'Astrologie Grecque," pp. 66 and 100.

[34] *Ibid.*, p. 100, note 5.

[35] *Ibid.*, p. 102. So also in modern astrology. See Ellen H. Bennett, "Astrology" (New York, 1897), p. 98.

in contradistinction to the other planets to whom generally a single dominant trait is given. Intelligence, thought, feeling, eloquence, artistic spirit are all associated with Mercury,[36] which thus becomes, as it were, the "soul" among the planets and it will not seem far-fetched to see in the fancy which makes Mercury the planet of revelation and of language[37] a trace of primitive views regarding the seat of vitality. In accord with this, we actually find Mercury assigned to the liver[38] as the organ of revelation, though in deference to later views of the liver as the seat of the affections specifically—and not of *all* intellectual life and of *all* emotions—Venus is sometimes identified with this organ.[39] To be sure, such associations of ideas have not as yet been encountered in Babylonian texts and therefore a certain reserve is called for. On the other hand, the dependence of Greek astrology on Babylonian conceptions, fancies and prototypes is so evident at every turn[40] that we are justified in assuming a large measure of identity between the two systems of divination, just as, on the other hand, modern astrology is full of conceits and notions that can be paralleled in ancient Greece, India and Persia.

Another factor that may have led to assigning to Mercury a specially prominent place among the planets for purposes of divination is the circumstance that by virtue of its close position to the sun and its small size, it makes its circuit in the short space of twelve weeks and four days, or 87.97 solar days. Hence, since the basis of divination in the case of the planets is largely bound up with their relative position to the sun—upper conjunction, ascent, culmination, standstill, descent, lower conjunction[41]—Mercury would present a far larger proportion of changes in any given time than any other planet. In the case of frequent observations, Mercury would thus play a more prominent part than the other planets whose movements except for periods of some duration would furnish less of moment

[36] Bouché-Leclercq, p. 101; Bennett, p. 99.
[37] Bouché-Leclercq, pp. 312, 321, 323.
[38] *Ibid.*, p. 312 and 323.
[39] *Ibid.*, p. 321.
[40] See Bouché-Leclercq's summary, pp. 70–71.
[41] See the valuable discussion in Kugler, *l. c.*, p. 20 f., of the Babylonian equivalents for those terms.

to the observer, dependent upon the naked eye. But whatever the reasons, we can only conclude from the fact that Mercury is the "sheep" *par excellence* that it was at one time singled out as *the* planet of revelation and that, therefore, it was in all probability the first planet whose movements were observed for the purpose of securing through them a means of determining what events the gods were preparing to take place on earth.

The designation of Saturn as *lulimu*, "ram," I am inclined to regard of secondary origin, that is to say, dependent upon the application of *bi-ib-bu* to Mercury—the latter term taken no longer in the sense of an "omen" but already as a specific and distinguishing designation. As companion piece, therefore, to Mercury as a "sheep," Saturn was called a "ram" just as the designation of the seven Maši-stars by the determinative LU ("sheep")[42] is a secondary extension from LU-BAT, limited originally to the planets. Saturn presents in almost every respect a contrast to Mercury. It is infinitely larger[43] in bulk, at a great distance from the sun, the most regular of the planets and the slowest in its motion, taking 10,759 days or 29.46 years to pass around the sun. In Greek astrology a preëminent position is accorded to Saturn,[44] which is expressed, for example, by making the planet the head and "brain" of the planetary world—reflecting the later view which placed the seat of the soul in the head,[45] while the association of Saturn with Mercury

[42] Kugler, *l. c.*, p. 7.

[43] Jupiter alone is larger.

[44] Bouché-Leclercq, *l. c.*, p. 94 f. It is to be noted that Saturn is in Babylonian astrology called "the star of the sun" (as Diodorus, II., 30, also says)—which reminds one that Mercury (see above, p. 152) was identified with the sun in Greek astrology; the same appears to have been the case with Saturn. See Kugler, *l. c.*, p. 8.

[45] *Ibid.*, p. 95. The soul was placed successively (*a*) in the liver, (*b*) in the heart and (*c*) in the brain. "Liver" divination is the outward expression corresponding to the first stage. The addition of the "heart" (and then of other organs) to the "liver" in the examination of the sacrificial animal—as among the Romans—is a concession to the second stage, while phrenology is an expression—outside of the official cult—of the third stage. See Jastrow, "Divination through the Liver and the Beginnings of Anatomy" (*University of Pennsylvania Medical Bulletin*, January, 1907). In a special paper on "The Liver as the Seat of the Soul" I propose to treat in detail of these successive views.

crops out in the belief which makes the history of the world begin with the reign of Saturn and end with that of Mercury.[46] The prominence of Saturn in Babylonian-Assyrian astrological texts is in accord with this association with Mercury as a second LU-BAT *par excellence*.[46a]

In modern astrology Saturn continues to play a particularly conspicuous rôle[47]—all of which points to its having been the first planet to become, by the law of contrasts, associated with the original "source" of divination among the planets—Mercury.

Lastly, a word regarding the ideographic designations of these two "sheep"—Mercury and Saturn. Kugler,[48] following in part Jensen,[49] proposes to take the element GU-UD in LU-BAT GU-UD as *ḳarradu ša urri*,[50] "warrior of the light," because shortly after his appearance in the East day triumphs over night. The explanation seems forced and it is hardly likely that a circumstance like this should have suggested a name for a planet. In view of the fact that Mercury and Saturn are the two planets more particularly designated as LU-BAT, it is more reasonable to see in LU-BAT GU-UD and LU-BAT SAG-UŠ descriptions of characteristic features. For SAG-UŠ, fortunately, the equivalent, *ka-a-ma-nu*,[51] has been definitely ascertained and the meaning "regular" is also beyond doubt. The name was clearly given to the planet because of the slow and regular motion which is its distinguishing feature. Mercury, on the

[46] Bouchè-Leclercq, pp. 187, 498 f.

[46a] The statement of Diodorus (*l. c.*) that Saturn was regarded by the Babylonians as the most important for purposes of divination may correctly reflect a later stage when Saturn assumed the preëminent place once occupied by Mercury.

[47] Bennet, *l. c.*, p. 93.

[48] Kugler, *l. c.*, p. 10. On p. 218 he prefers the rendering "full of light" (as Hommel, Aufsätze, p. 381, does) but the two ideas ("full" in the sense of "strong" and "warrior") are correlated.

[49] "Kosmologie," p. 131, who takes GUD-UD as a single term = *ḳarradu* "warrior" (Br. 5742). It is always to be born in mind that we are to substitute Mercury for Mars throughout Jensen's volume—now that it has been definitely ascertained that GUD-UD = Mercury and not Mars and ZAL-BAT (a-nu) = muštabarru mutânu = Mars not Mercury.

[50] GUD = ḳarradu and UD = urru (Br. 7798)—though ûmu = "day" would suggest itself as more probable.

[51] See Jensen, *l. c.*, p. 114. Cf. כיון in Amos 5, 26.

other hand, is marked by its rapid and irregular course and I accordingly propose the equation GU-UD = *šaḫâtu*—a common value of the compound ideogram in "liver" divination texts in the sense of "hinder, check, restrain."[52]

Assuming the adjective formation *šahtu* from this stem, the "checked" one as the designation of this planet would form a companion piece to *kaimanu*, the "regular" one. In contrast to *kaimanu* "regular," the designation *šaḫtu* would, naturally, convey the notion of a body checked and restrained and therefore "irregular" in its motion.[53]

[52] Cf. Jastrow, II., p. 366, note 9.

[53] The gloss in Hesychius according to which βελέβατος is in Babylonian the "fire" star cannot be explained as Jensen "Kosmologie," p. 97, proposes, since he starts from the false assumption—since abandoned by him—that *bibbu*—the LU-BAT *par excellence* is Mars, whereas it is Mercury. That βελέβατος designates Mars is however no doubt correct and since the common ideographic designation for Mars is ZAL-BAT—the addition of a-nu being a phonetic complement to suggest the phonetic reading *muštabarru mutânu*, "the one satiated with death"—the correction of βελέβατος to ζελέβατος suggests itself as a simple solution of the problem.

FURTHER RESEARCHES ON THE PHYSICS OF THE EARTH, AND ESPECIALLY ON THE FOLDING OF MOUNTAIN RANGES AND THE UPLIFT OF PLATEAUS AND CONTINENTS PRODUCED BY MOVEMENTS OF LAVA BENEATH THE CRUST ARISING FROM THE SECULAR LEAKAGE OF THE OCEAN BOTTOMS.

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I. General Considerations on the Physics of the Earth, with Especial Reference to the Secular Leakage of the Oceans and the Resulting Development of Mountains, Plateaus and Islands.

§ 1. *Introductory Remarks.*—In three papers recently communicated to the American Philosophical Society held at Philadelphia and since published in the proceedings of that Society,[1] the writer has treated at some length of the cause of earthquakes, moun-

[1] 1. "The Cause of Earthquakes, Mountain Formation and kindred phenomena connected with the Physics of the Earth," Proc. Am. Philos. Soc., 1906.

2. "On the Temperature, Secular Cooling and Contraction of the Earth, and on the Theory of Earthquakes held by the Ancients," Proc. Am. Philos. Soc., 1907.

3. "The New Theory of Earthquakes and Mountain Formation as illustrated by Processes now at work in the Depths of the Sea," Proc. Am. Philos. Soc., 1907; issued in March, 1908.

The following shorter articles have also appeared:

4. "Outline of the New Theory of Earthquakes," *Popular Astronomy*, April, 1908.

5. "How the Mountains were Made in the Depths of the Sea," *Pacific Monthly*, Sept., 1908.

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tain formation and kindred phenomena connected with the physics of the earth. In the course of these three memoirs many important questions are considered, and it seems to be rendered highly probable that six great classes of phenomena, not heretofore closely associated, depend on a single physical cause, namely, the secular leakage of the ocean bottoms, and the resulting movement of molten rock beneath the earth's crust. The six classes of phenomena traced to a single physical cause are: (1) world-shaking earthquakes; (2) the activity of volcanoes; (3) mountain formation; (4) the formation of islands and plateaus; (5) seismic sea waves; (6) the feeble attraction of mountains and plateaus long noticed in geodesy.

The first of the memoirs printed by the American Philosophical Society deals with the problem of earthquakes in its general aspects, and sets forth grounds for the theory that these six classes of phenomena are directly connected and dependent on a single physical cause; the second examines the question of the earth's temperature, secular cooling and contraction, and endeavors to show that the traditional theory of the changes noticed on the earth's surface is not well founded; while the third seeks to demonstrate the more important conclusions reached in the first memoir, by an appeal to processes now at work in the depths of the sea, the meaning of which apparently is so plain as to admit of no possible doubt.

The change in the point of view necessitated by the considerations brought forth in these papers is so remarkable as to be worthy of the attention of all who are interested in the grand science of natural philosophy. And we therefore propose to consider in this paper the physical basis of the theory of ocean leakage, the folding of mountain ranges and the uplift of plateaus and continents produced by movements of lava beneath the crust, together with the historical aspects of the problems of the physics of the earth. Heretofore the nature of the forces which have folded mountain ranges and their relationship to those slow movements which have raised whole continents have been equally mysterious and bewildering to the investigator. Accordingly any light which may be shed on this difficult subject will no doubt be exceedingly welcome to those who are interested in the progress of the physical sciences.

As the leakage of the oceans seems to be clearly proved by the movements noticed in earthquakes, especially where mountain formation is now going on in the depths of the sea, and the seismic disturbances are therefore accompanied by the sinking of the sea bottom, as shown by the seismic sea waves which follow the earthquakes, it seems legitimate to appeal to these movements of molten matter beneath the earth's crust as the only available means of demonstrating the porosity and other physical properties of layers of granite twenty miles thick. Owing to the restricted conditions of human life, no experiments on such a grand scale can ever be attempted in our laboratories, however great the facilities at our command; and our only means of ascertaining the truth with regard to the theory of ocean leakage is by careful observation in the great laboratory of nature. The leakage of the oceans involves three important questions: (1) The porosity of thick layers of matter such as those composing the earth's crust; (2) the penetrability of the crust under steady fluid pressure, by which the capillary forces are made to aid the molecular forces producing penetration of the fluid; and (3) the accumulation of stresses depending on the formation of steam in the layers just beneath the earth's crust.

The conditions existing in nature can scarcely be approximated in our laboratories, on account of the limitations of the forces at our command, but so far as experiments throw light on these great questions, the evidence tends to confirm the theory of ocean leakage. The well-known experiments of Daubrée, showing that under the action of capillary forces hot water will penetrate a layer of sandstone against a strong counter pressure of steam, and by entering a cavity actually increase the steam pressure on the further side, has been justly held to afford evidence of the leakage of the earth's crust, and of the probable mode of volcanic activity. If such action is possible in a minute way, it may easily operate on a vastly greater scale to produce the shaking of the crust in earthquakes, together with the uplift of mountains and the occasional outbreak of volcanoes.

Now if the rock of the earth's crust is at all as porous as we generally think, the constant pressure of the vertical column of

water, often miles deep, resting on the ocean bed must tend to force the fluid deeper and deeper into the bowels of the earth. A study of what takes place on our earth under the observed conditions constitutes therefore one of the grandest problems in natural philosophy.

Indeed it may be said that the great laboratory of nature has magnificent experiments constantly going on. All that we need to do is to interpret these experiments correctly. The best way to do this is to select phenomena in which the processes are so clear as to be free from doubt; after we have found the law of the phenomena in cases which are beyond question, we may then generalize and interpret other phenomena, in which the relations are not so obvious. By gathering principles and laws from cases which are entirely clear, and working by degrees to understand those which are more obscure, we may finally arrive at the true processes even when the operations of nature are quite hidden from our view.

Laws thus established by observation in the great laboratory of nature will obviously hold true of like experiments in the minute physical laboratories designed by man; and by noting the phenomena of the globe we may extend our knowledge of the universal properties of matter under various physical conditions often more extreme than those ordinarily witnessed at the surface of the earth.

§ 2. *Heretofore the ocean bottoms have been assumed to be water-tight.*—The earth's crust is made up chiefly of sedimentary, igneous and granitic rocks, and soil produced by the decomposition of the various kinds of rock under the action of water and the atmosphere. Nearly all of the sedimentary rocks are quite leaky, and moreover they absorb a great deal of moisture from the air; the formation of artesian wells and of natural springs depends primarily upon the percolation of water through rocks and layers of soil of various kinds. The leaky character of the sedimentary rocks is well known and has been generally recognized. But these rocks exist only near the surface, and do not extend more than a very few miles deep; consequently they could admit the water to but a slight depth into the earth's interior. Below the sedimentary rocks lies the mass of granite which makes up by far the greater part of the earth's crust. The granitic rocks, such as granite, andesite, dia-

base, etc., are by no means so penetrable as the sedimentary rocks, and hence water has more difficulty in passing through them. And as the layers of this material composing the earth's crust are about fifteen miles deep, it has been generally held that water would have difficulty in making its way down into the heated layers just beneath the crust.[1] Indeed it has been practically assumed that the ocean bottoms are water-tight, in spite of the great fluid pressure constantly exerted by the mere depths of the water over a large part of the bottom of the sea. This fluid pressure in many places is great enough to throw a column of water to the free surface, over five miles high; and it operates not only from day to day, year to year, but also from century to century, age to age. If granite is at all penetrable by water, is it therefore any wonder that a gradual secular leakage should go on, and at length, by a kind of slow perspiration of the stone, give rise to sufficient accumulation of steam beneath the crust to produce a swelling of the saturated mass, and require a readjustment of the overlying rocks?

Now it happens that by nature all the granitic rocks are crystalline, and thus somewhat coarse-grained in structure; so that they absorb water from the ground and moisture from the air. The crystalline structure permits penetrability to a greater degree than would fine-grained and very hard rocks such as agate; but no rock has such fine pores as the metals, and especially vitreous bodies like glass, to which agate is an approximation. And as all the metals are proved by experiment to be leaky under great fluid pressure, and glass is shown to obey the same law, it obviously follows that all rocks are leaky under great fluid pressure. Consequently under the incessant pressure of the oceans water must make its way into the heated layer just beneath the earth's crust.

Heretofore the possibility of earthquakes cracking the ocean bottom has been generally recognized, but it has been held that

[1] This statement is perhaps too positive, for Sir William Ramsay, the celebrated British Chemist and Physicist, writes me that he has long believed that the ocean bottom leaks and that the formation of minerals takes place chiefly in the bed of the sea. Undoubtedly this view will come to be generally accepted. Similar views seem to be held by Lord Rayleigh, Sir Wm. Huggins, Arrhenius and many other eminent physicists.

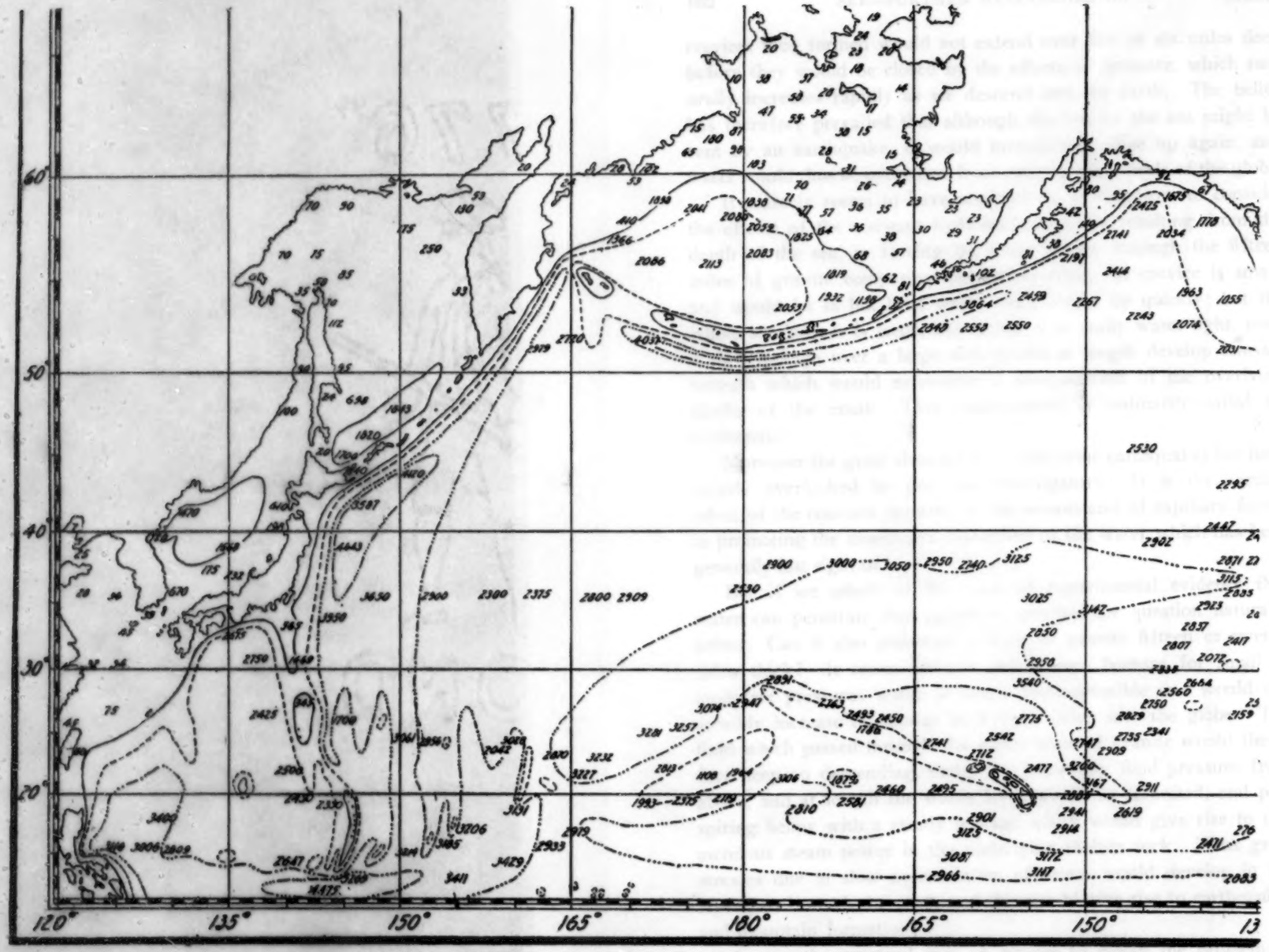
crevices thus formed would not extend over five or six miles deep before they would be closed by the effects of pressure, which naturally increases rapidly as we descend into the earth. The belief has therefore prevailed that although the bed of the sea might be rent by an earthquake, it would immediately close up again, and water would thus be prevented from entering the bowels of the globe.

It scarcely seems to have occurred to investigators to consider the effects of the constant hydrostatic pressure resulting from the depth of the sea, in forcing the water slowly through the fifteen miles of granite composing the earth's crust. A crevice is small, and would let in but little water when closed up quickly; but the whole sea bottom is large, and unless it is really water-tight, even a slow leakage over a large area would at length develop stresses beneath which would necessitate a readjustment of the overlying blocks of the crust. This readjustment is ordinarily called an earthquake.

Moreover the great abundance of submarine earthquakes has been largely overlooked by previous investigators. It is the secular effect of the constant pressure of the oceans and of capillary forces in promoting the downward movement of the water which has been generally lost sight of.

But if we admit on the basis of experimental evidence that water can penetrate thin layers of granite, the question naturally arises: Can it also penetrate a layer of granite fifteen or twenty miles thick? It seems obvious that it can, because for small or moderate pressures water is nearly incompressible and would not sensibly increase in density as it went down into the globe. The fluid which passed through the upper layer of granite would therefore keep on descending, under the increasing fluid pressure from above, and at length the whole layer would be saturated, and perspiring below with a steady leakage which would give rise to tremendous steam power in the underlying molten rock. Thus great stresses due to slow accumulation of steam would develop in the layer just beneath the crust, and this would give rise to earthquakes and mountain formation.

Among the practical men of science to whom the problem of ocean leakage was submitted, we might name some of the most



Coast Survey map of the North Pacific Ocean, showing the deep trenches in the sea bottom dug out by earthquakes in uplifting the Aleutian, Kurile and Japanese Islands. In the contour lines 4 points denote a depth of 4,000 fathoms, 3 points 3,000 fathoms, the continuous dash 2,000 fathoms and the continuous line near shore 100 fathoms.

eminent of living physicists. While disclaiming especial authority to pass upon such a question, they expressed the opinion that it was very improbable that the ocean bottom could be water-tight, unless the nature of the rock was greatly modified by pressure, which could hardly be the case in the first twenty miles of the earth's crust, where the pressure does not exceed 8,600 atmospheres.

Whatever doubt might attach to this solution of the problem, from an experimental standpoint, where positive knowledge is greatly lacking, seems to be dispelled by the phenomena noticed in the sea bottom in various places, which show that lava is expelled from beneath the sea and pushed under the adjacent land. The phenomena noticed in the laboratory of nature thus prove the leakage of the ocean from an observational standpoint, because they admit of no other interpretation.

§ 3. *The Theory of Water-tightness of the Ocean Bottoms Disproved by the Expulsion of Lava from under the Sea.*—Just south of the Aleutian Islands, a long, narrow and deep trench just parallel to this chain has been dug out by the expulsion of lava from beneath the sea. The nature of this trough is illustrated by the accompanying Map.

It will be seen that the island chain adjacent to the trough dug out in the sea bottom is really a mountain range under water, with only occasional peaks projecting above the water as islands. In fact the Aleutian Islands are a continuation of the Alaskan Mountains which are part of the Rocky Mountain System, and the range here continues into the sea. If therefore the Aleutian Islands are mountains now in process of formation in the sea, it would seem to follow logically that the Rocky Mountains and Andes, from Alaska to the straits of Magellan, were formed in the same way. What then is the process at work forming the Aleutian Islands?

It is evident that the deep trench south of the islands has been dug out by the expulsion of lava from under the sea and its injection under the Aleutian ridge; this is accomplished by earthquakes, and the process is still in full operation at the present time. This region is a well-known breeding-ground for world-shaking earthquakes and seismic sea waves. Several islands have been uplifted since 1783, and one or more new volcanoes have broken out within

the historical period. The seismic sea waves following the earthquakes which affect this region indicate that the sea bottom often sinks after these disturbances. In other words, when lava is expelled from under the trench and pushed under the adjacent ridge, the bottom gives down to secure stability. The processes now going on have been at work through immense ages, and have thus dug out the trough parallel to the Aleutian Islands, and at the same time elevated this ridge, till it is now partly above the water, thus constituting the chain of islands.

In like manner the great earthquake at Yakutat Bay, farther east, September 3–20, 1899, which was so carefully investigated by Tarr and Martin (*Bulletin of the Geological Society of America*, May, 1906) raised the coast for about 100 miles; the maximum elevation being 47⅓ feet. Subsidence also occurred in a few places. Such a vast movement of the coast indicates an enormous expulsion of molten rock from beneath the sea under the land. It is these subterranean movements beneath the earth's crust which shake down cities and devastate whole countries. During the earthquake at Yakutat Bay the shaking was so terrible that persons could not stand on their feet; avalanches slid down the mountains, and glaciers were carried into the sea. This is the true nature of earthquakes, and one need not therefore be surprised at the devastation produced. The force which pushes lava under the land, overcoming the weight of the crust, naturally destroys cities and all the frail works of man built upon the surface.

§ 4. *Physical Experiments on the Porosity of Matter.*—Modern science presents many illustrations of the *porosity* of matter. In fact so many experiments illustrate *porosity* that it is difficult to find proof of the general property of *impenetrability* cited by Newton in the "Principia," except under the narrow limitations that the matter in question remains cold and the forces to which it is subjected are small. With increasing fluid pressure and rising temperature all matter is leaky; and in general a rise of temperature expands and thus augments the penetrability and porosity of all substances. *We may therefore say that all matter is porous and leaky under great fluid pressure, and impenetrability does not exist except*

under very restricted conditions, so that it is not a general property of matter as was once supposed.

In the early days of physical science the demonstration of the porosity of such dense bodies as gold, silver and lead was considered a great achievement. In 1661 some academicians at Florence, repeating an earlier experiment of Bacon with a spherical shell of lead, filled a hollow sphere of solid gold with water, and, after sealing it hermetically, flattened the figure of the spherical shell in a hydrostatic press so as to diminish the volume. Under this deformation of the sphere the water was forced through the walls of solid gold and formed in drops on the outside. Corresponding experiments were made with spheres of silver, lead, and other metals, with analogous results. Modern engineering presents innumerable illustrations of the porosity and leaky character of structures made of the hardest bodies. Under great pressure all pipes and pistons leak, and put a limit to the applications of hydrostatic pressure.

In 1883 Amagat forced mercury through plates of solid steel three inches thick, under a pressure of about 4,000 atmospheres. This is the highest pressure hitherto applied in physical experiments, and yet all rocks are subjected to such pressure at a depth of only ten miles below the earth's surface. In the measurement of ocean depths it has been found that empty hollow glass balls with walls half an inch thick sent down with the deep sea apparatus come up more and more completely filled with water, according to the depth of the sea and the duration of the experiment. As glass is the most impervious of solid bodies, this leakage, which it shows under the external application of fluid pressure from the deep sea, is a good illustration of what happens to the bed of the ocean, which is constantly subjected to this pressure. No rock is anything like so impervious as glass, and consequently a general leakage of the ocean bottom inevitably takes place. The water which first enters the bed of the sea will keep on descending till it comes into contact with rock at high temperature, which produces and readily absorbs steam. When the rock becomes saturated with steam it swells and requires more space, and this finally brings on an earthquake. Hence also the preponderance of great earthquakes under the sea and the almost total absence of these disturbances far inland.

§ 5. *Important Criterion for the Nature of the Movement Beneath the Earth's Crust furnished by Seismic Sea Waves.*—In the paper on the "Cause of Earthquakes" we divided seismic sea waves into two general classes: the first, due to the sinking of the sea bottom, and characterized by a withdrawal of the water after the earthquake, to be followed later by the return of a great wave; the second, due to the uplift of the bottom, and characterized by the sudden rise of the sea without any previous withdrawal from the shore. Both classes of these waves exist in our seas, but those of the first class are the most dangerous and the most important. Most of the great historical inundations by the sea have been due to waves of the first class. The phenomena usually noted are: first, a terrible earthquake; second, after a short interval, the sea is noticed to be slowly draining away, laying bare the bottom, where it ordinarily is deep enough to anchor ships; third, after an interval of an hour or so, the sea is seen to be returning as a mighty wave, carrying everything before it, and thus washing the ships inland and stranding them high and dry; fourth, having once swept the shore, the sea again withdraws and lays bare the harbor as before, and after about the same interval again returns as a second great wave. This periodic movement of the sea may be kept up for quite a while, and sometimes quiet is not restored for a day or two.

Among the many well-known historical sea waves of the first class which might be mentioned, we shall cite only a few typical cases: As that which overwhelmed Helike in 373 B. C. (see the paper on the "Temperature of the Earth," § 23, pp. 269–272, and Addendum, pp. 291–298); the wave at Callao in 1746; the wave following the Lisbon earthquake in 1755; the waves of Arica, 1868, and Iquique, 1877; the wave on the Japanese coast in 1896. In all these cases the water first withdrew from the shore; not suddenly, but slowly, as in the draining away of a tide, though somewhat more rapidly; this of course indicated that the sea bottom had sunk, and the water was draining away to fill up the depression in the level caused by the drop of the bottom. When the currents meet at the center an elevation is produced by their mutual impact, and

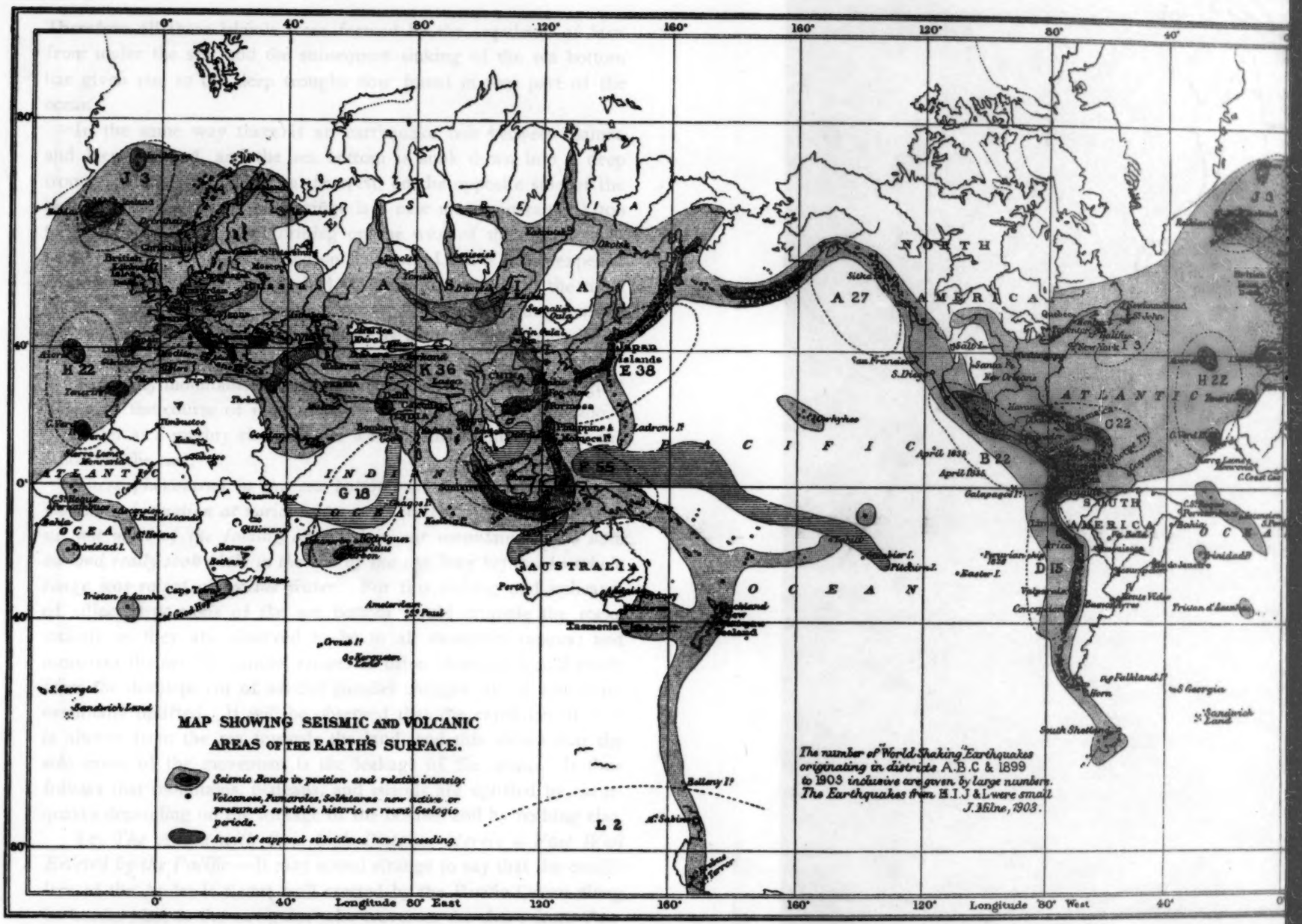
when this collapses under gravity the first great wave comes ashore. The elevation then subsides into a depression as at first, and the currents again flow in and force up the level a second time; and with the second collapse another wave is sent ashore; and so the oscillation of the sea continues, sometimes for a day or two before it finally quiets down.

Now these sea waves of the first class furnish an exceedingly important criterion as to the nature of what is going on beneath the earth's crust. The sinking of the sea bottom often happens in the deep trench south of the Aleutian Islands, and repeated drops of this kind have obviously produced the deep trough parallel to these islands. For it is observed that the earthquake usually raises one or more of the islands to the north, when the sea bottom sinks to the south. Now the islands could not be upraised unless something was pushed under them, and the bed of the trough could not sink down unless it was in some way undermined. Accordingly it follows that molten rock is expelled from beneath the bed of the trough to the south and pushed under the adjacent islands to the north, which are thus uplifted. The bed of the sea often sinks during the earthquake arising from this subterranean movement, and then the water withdraws from the shore and afterwards returns as a great seismic sea wave. It will be observed that the subcrustal movement is from the sea towards the land, because steam accumulates under the ocean, but scarcely at all under the land.

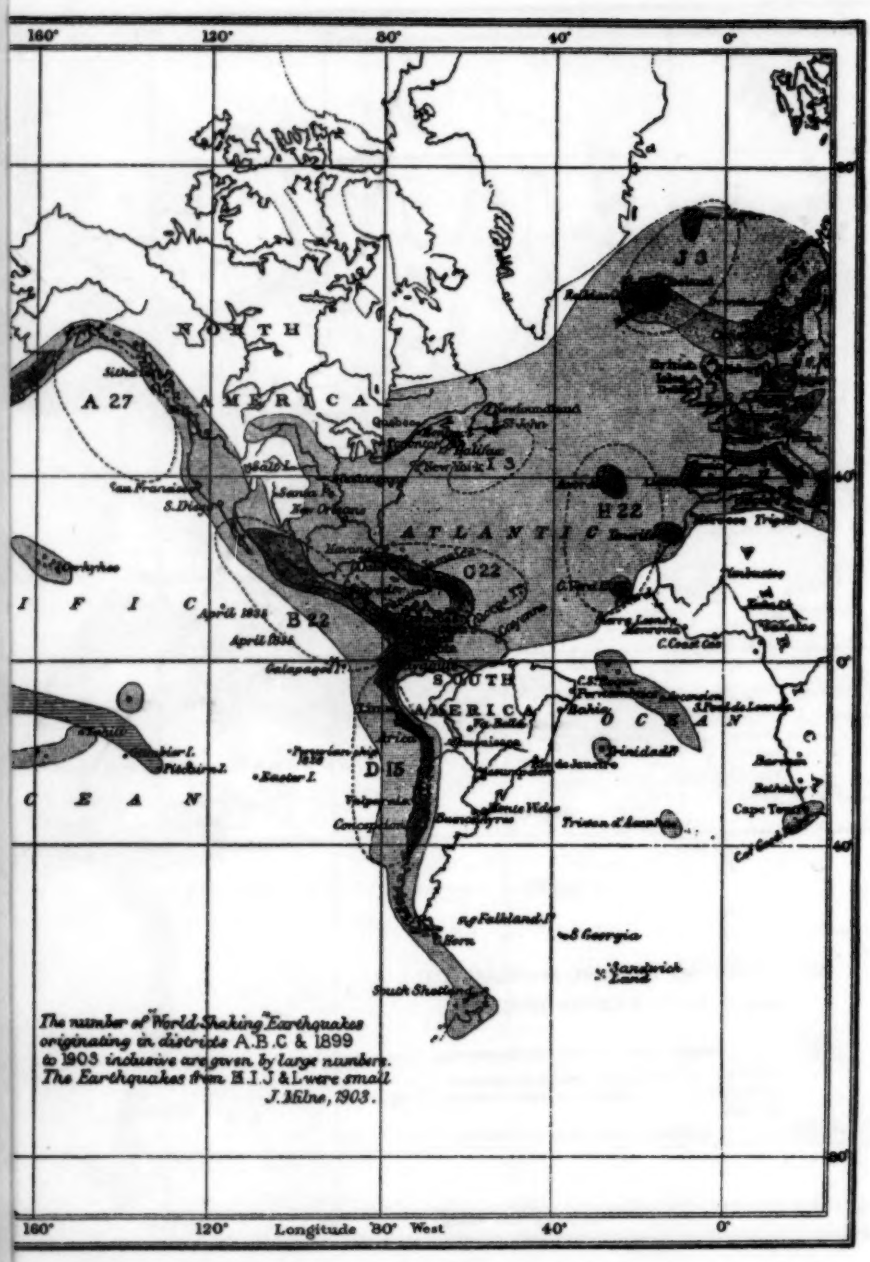
Thus these seismic sea waves become very important criteria for determining whether the sea bottom has sunk; and if it has sunk we know that lava was expelled from under the sea and pushed under the land. Seismic sea waves therefore may be regarded as *very delicate levels,* for determining the movement of the sea bottom; and from the nature of this movement we can often decide what the effect of the earthquake has been. Moreover these waves enable us to tell with certainty that the chief function of earthquakes is the elevation of the land along the coast by the expulsion of lava from beneath the bed of the sea. It is not too much to say that the true nature of earthquakes and their function in the uplift of mountains and plateaus could not be certainly made out except for the exceedingly important criterion furnished by seismic sea waves.

§ 6. *Additional Phenomena Noticed near the Aleutian, Kurile and Japanese Islands, and the Antandes.*—South of the Aleutian chain, as just remarked, a well-known earthquake belt parallels these islands, and the seismic disturbances occurring there are frequently followed by seismic sea waves of the first class. Soon after a great earthquake the water is seen to be withdrawing from the shore, and after a short interval of time it again returns as a mighty wave sweeping everything before it. Many volcanoes have broken out in these islands and several new islands have been uplifted within the historical period. The Russians long ago connected the earthquakes with the volcanoes in the Aleutian Islands. In later years the exact survey of the sea bottom has shown that it is sunk down into a narrow trough right under the earthquake belt. Just parallel to the trough the islands form a real mountain ridge under water, with only a few of the highest points projecting above the surface as islands. The uplift of these islands therefore denotes the uplift of mountain peaks, some of which have become volcanoes.

Now if the earthquakes are accompanied by the uplift of islands and the sinking of the sea bottom, as shown by the seismic sea waves, it follows that the uplift of the ridge is connected with the sinking of the adjacent sea bottom. As the ridge is just contiguous to the trench, and the earth is terribly shaken every time these disturbances occur, it seems to indicate that matter is expelled from under the trench and pushed under the ridge; so that the ridge is elevated and the trench sinks down correspondingly. This could not occur without the bodily transfer of matter beneath the earth's crust, and the shaking of the earth is due to this expulsion of lava from under the trench, and its injection under the ridge. This is the only possible explanation of the observed elevation of the ridge and the sinking of the trench. In this way the trough near the Aleutian Islands has been gradually dug out. Similar troughs have been formed by earthquakes near the Kurile and Japanese Islands, as we know by the observed depth of the sea, the lay of the earthquake belt parallel to these islands, and the occurrence of the seismic sea waves, showing that the sea bottom sinks after the earthquakes by which the region is afflicted. If the islands of Japan were dug off and thrown into the Tuscarora Deep, they would about fill it up.


MAP SHOWING SEISMIC AND VOLCANIC AREAS OF THE EARTH'S SURFACE.
Seismic Bands in position and relative intensity.
Volcanoes, Fumaroles, Solfataras now active or presumed as within historic or recent Geologic periods.
Areas of supposed subsidence now proceeding.
The number of "World-Shaking" Earthquakes originating in districts A.B.C & 1899 to 1903 inclusive are given by large numbers. The Earthquakes from E.I.J & L were small.
J. Milne, 1903.
Longitude 80° East
Longitude 80° West
ATLANTIC OCEAN
INDIAN OCEAN
PACIFIC OCEAN
NORTH AMERICA
SOUTH AMERICA
AUSTRALIA
A 27
B 22
C 22
D 15
E 38
F 55
G 18
H 22
J 3
K 36
L 2

Milne's map of earthquakes, 1899–1903; from his work on "Earthquakes," London, 1903. More seismic disturbances occur in the sea bottom than are indicated, because large parts of the ocea observed. The prevalence of the great earthquake belt about the Pacific Ocean, however, is very conspicuous and of the highest significance.



nces occur in the sea bottom than are indicated, because large parts of the ocean are but little
lowever, is very conspicuous and of the highest significance.

Therefore all these islands were formed by the expulsion of lava from under the sea, and the subsequent sinking of the sea bottom has given rise to the deep troughs now found in that part of the ocean.

In the same way there is an earthquake belt between Samoa and New Zealand, and the sea bottom is sunk down into a deep trough, parallel to a ridge on the west, on the opposite side of the trench from the ocean. This ridge is a new mountain range 1,200 to 1,500 miles long, now forming on the west of the Pacific, just as the Andes were once formed on the east. Lava is being expelled from under the trench and pushed from the ocean towards the ridge on the west. This is developing into a new mountain range, which we shall call the *Antandes,* because it is being formed opposite to the Andes, on the other side of the Pacific, and in the same manner as the mighty mountains in South America were in earlier geological time. In the course of immense ages the Antandes will rise above the water as a mighty chain on the west of the Pacific just like the Andes on the east.

These phenomena in the sea bottom show the real process of mountain formation at various stages of its progress, and prove to us that most of the folding observed in our mountain ranges now on land really took place in the bed of the sea, long before the whole range was raised above the water. For this sinking and upheaval of adjacent portions of the sea bottom would crumple the rocks exactly as they are observed to be in all mountain ranges; and moreover the several parallel ranges so often observed would result from the development of several parallel troughs, all of which are eventually uplifted. It will be observed that the expulsion of lava is always from the sea towards the land, and this shows that the sole cause of the movement is the leakage of the ocean. It thus follows that mountains, plateaus, and islands are uplifted by earthquakes depending on the leakage of the oceans, and by nothing else.

§ 7. *The Andes with their high Plateaus Merely a Vast Wall Erected by the Pacific.*—It may sound strange to say that the cordillera of the Andes is a vast wall erected by the Pacific Ocean along its border; but to the navigator who traverses the shore from Panama to Cape Horn such a description will seem most appropriate.

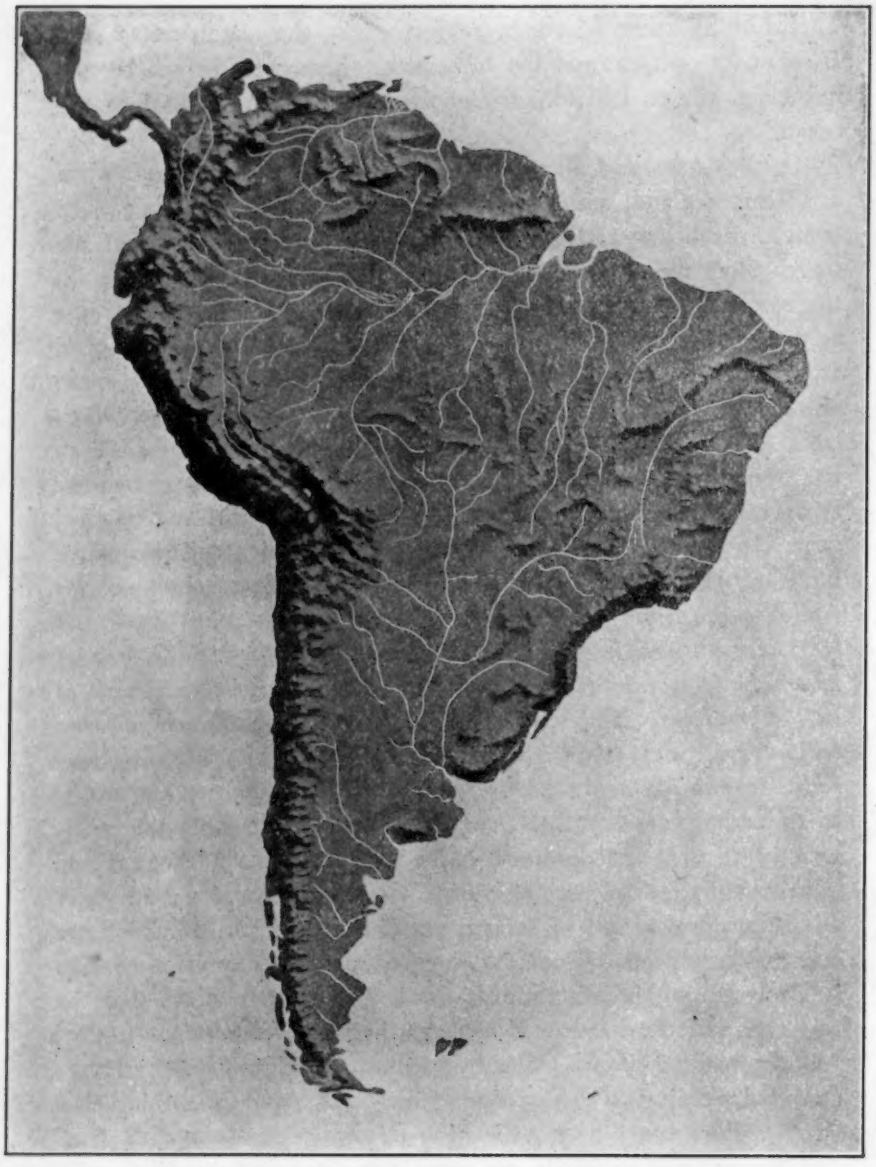


FIG. 1. Relief Map of South America. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) Notice that the Andes are a mighty wall erected by the Pacific Ocean along its border. Professor Charles Burkhalter, Director of the Chabot Observatory, Oakland, kindly suggested the use of these relief maps, which are well suited for bringing out the leading characteristics of the different continents.

Throughout the length of the continent the mountains are everywhere parallel to the coast, and run at nearly a constant distance from the shore. The Andes are not always a simple chain, but they are narrow relatively to their height, as compared to the other mountains. In many places there are two or more ranges with narrow plateaus between. These plateaus are so interwoven with the mountains themselves that we may feel sure they were formed together and represent a part of one general movement. Unless this were so it is impossible to believe that so many narrow and high plateaus would be enclosed between mountain walls on either side. The eastern cordillera is less volcanic than the western, and the eastern slope is believed by Professor Solon I. Bailey of Harvard College Observatory, who has exceptional opportunities for judging of these mountains, to be two or three times steeper than the western slope.

If we suppose a sea trough was first dug out in the elevation of the eastern range, and eventually when deep sediments had accumulated in the trough, the western edge of it was folded up to form the western range, and the trough itself became the plateaus, we shall have very nearly a true picture of how the Andes were formed. The full details of this process cannot be given now, but there is no doubt that the Andes are a vast wall erected by the Pacific along the edge of the continent. This origin of these mountains is also indicated by the earthquakes observed within historical time; for the coast has been again and again upraised by these disturbances, while the sinking of the sea bottom, indicated by the accompanying seismic sea waves, shows that the bed of the ocean is being undermined by the expulsion of lava under the land. The shells, fossils, and other evidences of marine life now found at altitudes as high as 15,000 feet show that the uplifting at present going on is but a part of the greater uplift of past geological ages; so that the great movement which formed these mountains and plateaus is identical with the earthquake disturbances noticed within historical time.

§ 8. *The uplift of mountains and plateaus around the margins of the Pacific, and of islands in the interior, with innumerable submarine eruptions everywhere, is nature's way of indicating leakage*

through twenty miles of crust.—The peculiar position of the sea bottom between a molten globe and the overlying ocean is such that any disturbance of the bottom, as in a volcanic eruption, would naturally excite our suspicions that the crust had leaked and brought the water into contact with the underlying ball of fire. The situation of the overlying ocean, with the fire so close beneath, is much the same as that of the water above the furnace of a boiler, in which steam is developed; and if one had the molten globe for a furnace and the ocean for a reservoir of water, leakage would develop steam on a grand scale, and give rise to mighty experiments exactly resembling earthquakes and volcanic eruptions. Some of these disturbances might take the form of uplifts of the crust into islands, mountains and submarine volcanoes, others near the edges of the sea would cause lava to push out under the land and raise the coasts.

Now the Pacific Ocean is everywhere surrounded by high mountains, as if the lava had been pushing out at the margins of the sea. And throughout the interior a vast number of islands are raised up in deep water, and every part of the ocean is from time to time disturbed by terrible earthquakes. One must therefore admit that the ocean has the aspect which might be expected to result from a leakage of the ocean bed. Moreover the Pacific is surrounded nearly everywhere by volcanoes, which emit chiefly vapor of steam. If it is shown that mountains are formed by earthquakes, chiefly in the expulsion of lava under the land, and some of the mountains break out into volcanoes, then there will obviously be a connection not only between earthquakes and volcanoes, but also between the vapor of steam emitted from these smoking mountains and that formed under the ocean by the leakage of the bottom.

It is this intimate connection between all the related phenomena which tells so powerfully in favor of the view that the leakage of the ocean takes place through a layer of rock twenty miles thick. The height of the mountains and plateaus is but a small fraction of the thickness of the crust, and movement in the underlying layers therefore usually gets relief without breaking through. The crust of the globe is thick enough to offer great resistance to uplift, so that the steam saturated lava usually adjusts itself beneath without a surface eruption. Yet where the crust is sharply upheaved as in

mountains, volcanoes sometimes break out. But it is obvious that earthquakes are the more general, volcanoes the more special phenomena; and that both are connected with mountain formation, and depend on the sea for their continued activity.

§ 9. *On the Structure of Granite as a Typical Crystalline Rock of the Earth's Crust.*—Granite has a thoroughly crystalline structure, and is an admixture of feldspar, mica, and quartz. The mica is in the form of minute shingles, or snowflakes, embedded in the non-crystalline matrix of quartz, which encloses the other elements. The feldspar is chiefly orthoclase. The two chief ingredients, quartz and feldspar, form a granular aggregate made up of grains of fairly equal size, varying all the way from several inches in diameter to a structure so fine as to be inseparable to the naked eye.

"Many granites contain irregularly shaped cavities (miarolitic structure), in which the component minerals have had room to crystallize in their proper forms, and where beautifully terminated crystals of quartz and felspar may be observed. It is in these places also that the accessory minerals (beryl, topaz, tourmaline, garnet, orthite, zircon and many others) are found in their best forms. Not improbably these cavities were somewhat analogous to the steam holes of amygdaloids, but were filled with water or vapour of water at high temperature and under great pressure, so that the constituents could crystallise under the most favorable conditions. Among the component minerals of granite, the quartz presents a special interest under the microscope. It is often found to be full of cavities containing liquid, sometimes in such numbers as to amount to a thousand millions in a cubic inch and to give a milky turbid aspect to the mineral. The liquid in these cavities appears usually to be water, either pure or containing saline solutions, sometimes liquid carbon-dioxide" (p. 143). (Sir A. Geikie, "Geology," p. 204.)

The cavities in crystalline rock such as granite may contain either gas or liquid matter, and sometimes both. Professor Tilden's researches have shown that the included gases (hydrogen, carbon dioxide, carbon monoxide, marsh gas, nitrogen, and water vapor) may exceed many times the volume of the rock itself. The cavities have all manner of forms, branching, oblong, curved, oval, spherical and negative crystalline shapes, and are often so numerous as to give a turbid aspect to the mineral. The intersecting planes of the crystalline granite frequently present real fissures more or less filled with liquid. Obviously capillary forces may here attain great importance, and fluid entering the rock would be absorbed into these spaces

with irresistible power. Geikie remarks that the cavities in quartz have all sizes from the coarse pores visible to the naked eye to minute spaces less than 1/10,000 of an inch in diameter, which can be seen only under high magnifying power.

Now it is worth while to remember that small as are the least cavities and fissures which we can see with the microscope, they are very large and coarse compared to the molecular structure of a fluid such as water or of a solid like glass. It is useful to remember that the limit of naked eye vision is about 1/250 of an inch, and of the most powerful microscope about 1/100,000 of an inch. The microscope therefore increases our power of vision about 400 times. (Cf. Prof. A. A. Michelson's " Light Waves and their Uses," p. 30.)

§ 10. *On Lord Kelvin's Determination of the Size of Atoms.*— In order to form a clear conception of the physical constitution of the matter composing the crust of the globe, we must recall the lines of research by which Lord Kelvin has determined the size of atoms.

1. By determining the work done or heat produced in bringing thin plates of zinc and copper together. The observed amount of heat evolved when the plates are made of given thickness and afterwards imagined to be thinner and thinner, limited only by the condition that the mass shall not be melted, under the heat of combination, which is not indefinitely great even when brass is produced by fusing zinc and copper, but corresponds to the mutual attraction of a number of plates not more numerous than 100,000,000 to the millimeter; hence it follows that the molecules are at least 1/1,000,000,000 cm. and probably more than 1/400,000,000 cm. in diameter. Lord Kelvin concluded that " Plates of zinc and copper 1/300,000,000 of a centimeter thick, placed close together alternately, form a near approximation to a chemical combination if indeed such thin plates could be made without splitting atoms." He fixed 1/1,000,000,000 of a centimeter as the minimum diameter of the atoms found in this way. It is to be remembered here that 2.54 centimeters = 1 inch.

2. By the study of Newton's rings on soap bubbles as they become thinner and thinner, the thickness of the film being reckoned from the known wave-length of the reflected light. Unless the film materially weakened when a certain limit is attained, it could not be stretched beyond a certain thickness without volatilizing, if

maintained at the same temperature; for as it expands it cools, and the heat that would have to be supplied to it would be more than sufficient to vaporize it. Now it is found by observation that the intensity of the surface tension of the film of water falls off before the thickness is reduced to 1/200,000,000 cm., and hence there probably are but few molecules in that thickness.

3. By the phenomenon of dispersion in the wave theory of light. Cauchy showed that dispersion of colors implied a granular structure in refracting media, and that the grains could not be indefinitely small, but must exceed 1/10,000 of the shortest wave length; and to produce the observed effect Lord Kelvin concluded that the number of molecules in a wave length would have to be from 200 to 600. Nobert ruled lines on glass at the rate of 40,000 to the centimeter,[2] or about two to the wave length of blue light (about 4/100,000 centimeter); and as this left the ruled surface capable of reflection, the number of molecules in the ridges between the grooves must have been sufficient to give solid body to the sculptured mass, and thus not less than several hundred to the wave length. If the mean free path in a solid like glass be 25 times the diameter of the atom itself, this will make the diameter of the atoms of the order of 1/400,000,000 of a centimeter.

4. By calculating the length of the average free path of a molecule in a gas, according to the kinetic theory. Loschmidt in 1865, Stoney in 1866, and Lord Kelvin in 1870, independently reached similar results, namely, for the average free path about 1/100,000 of a centimeter, and for the diameter of the gaseous molecule about 1/500,000,000 of a centimeter.

These four methods of estimating the diameter of atoms thus agree very closely among themselves; and moreover a similar result on the average distance of molecules deduced by entirely different

[2] Referring to Nobert's lines Maxwell says: "A cube, whose side is the 400th of a millimetre, may be taken as the *minimum visible* for (microscopic) observers of the present day. Such a cube would contain from 60 to 100 million molecules of oxygen or nitrogen" (cf. The article "Atom," Encyclopedia Britannica, ninth edition, p. 42). If there be 400 molecules in a line the length of the edge of the cube just considered, the cube would contain 64,000,000, which agrees with Maxwell's estimate. A line equal to the wave length of blue light would thus contain 250 molecules.

considerations was obtained by M. Lippmann, in a paper read to the Paris Academy of Sciences, October 16, 1882.

In his "Popular Lectures and Addresses" (vol. 1, p. 224) Lord Kelvin condenses his conclusions as follows:

"The four lines of argument which I have now indicated lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they established, with what we cannot but regard as a very high degree of probability, the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the 1/5,000,000 and greater than the 1/1,000,000,000 of a centimeter.

"To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a globe of water or glass, as large as a football,[1] to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse grained than a heap of small shot, but probably less coarse-grained than a heap of footballs."

§ 11. *On the Molecular Constitution of Matter and on the Penetrability of Solids by Fluids.*—In his address on "Mathematical Physics" at the St. Louis Congress of Arts and Sciences in 1904, Poincaré speaks of the porosity of matter as follows:

"The astronomical universe consists of masses, undoubtedly of great magnitude, but separated by such immense distances that they appear to us as material points; these points attract each other in the inverse ratio of the squares of their distances, and this attraction is the only force which affects their motion. But if our senses were keen enough to show us all the details of the bodies which the physicist studies, the spectacle thus disclosed would hardly differ from the one which the astronomer contemplates. There too we should see material points separated by intervals which are enormous in comparison with their dimensions, and describing orbits according to regular laws. Like the stars proper, they attract each other or repel, and this attraction or repulsion, which is along the line joining them, depends only on distance." (Cf. *Bulletin of the American Mathematical Society*, February, 1906, p. 241; authorized translation by Professor J. W. Young.)

Professor Sir G. H. Darwin's recent presidential address to the British Association for the Advancement of Science at Cape Town, 1905, was devoted largely to the discovery of electrons. After treating of these subatomic corpuscles he adds:

"I have not as yet made any attempt to represent the excessive minuteness of the corpuscles, of whose existence we are now so confident; but, as an introduction to what I have to speak of next, it is necessary to do so.

[1] Or say a globe of 16 centimeters diameter.

To obtain any adequate conception of their size we must betake ourselves to a scheme of threefold magnification. Lord Kelvin has shown that if a drop of water were magnified to the size of the earth the molecules of water would be of a size intermediate between that of a cricket ball and of a marble. Now each molecule contains three atoms, two being of hydrogen and one of oxygen. The molecular system probably presents some sort of analogy with that of a triple star; the three atoms replacing the stars, revolving about one another in some sort of a dance which cannot be exactly described. I doubt whether it is possible to say how large a part of the space occupied by the whole molecule is occupied by the atoms; but perhaps the atoms bear to the molecule some such relationship as the molecule to the drop of water referred to. Finally, the corpuscles may stand to the atom in a similar scale of magnitude. Accordingly, a threefold magnification would be needed to bring these ultimate parts of the atom within range of our ordinary scales of measurement. . . .

"The community of atoms in water has been compared with a triple star, but there are others known to the chemists in which the atoms are to be counted by fifties and hundreds, so that they resemble constellations."

Such general discussions by these illustrious physicists, Kelvin,[1] Poincaré and Darwin, are not to be construed too literally, and yet they clearly indicate the general belief among the foremost men of science that the spaces between the particles of matter are immense in comparison with the dimensions of the particles themselves.

From Lord Kelvin's discussion of the size of atoms treated in the above section, we have seen that the diameters of these bodies is of the order of 1/500,000,000 of a centimeter, or 1/1,270,000,000 of an inch. The average space between the molecules being 1/100,000 of a centimeter, or about 5,000 times the diameter, is of the order of 1/254,000 of an inch. This is decidedly below the

[1] In a well-known paper on gravitating matter, Lord Kelvin compares the stars of the Milky Way to the atoms of a bubble of gas. For a giant for whom our suns would be what atoms are to us, the stars would be beyond the reach of the keenest vision and the Milky Way appear to behave as a gaseous medium. M. Poincaré has discussed the problems of the universe from this point of view in an address to the Astronomical Society of France (*Bulletin Astronomique de la Societé Astronomique de France,* April, 1906; an excellent translation in *Popular Astronomy* for October, 1906). It is remarkable that Democritus, founder of the atomic theory among the Greeks (460–360 B. C.), should also have recognized that the Milky Way is composed of a mass of stars too dense to be seen separately by the unaided vision (cf. "Aristotle's Meteorology," Lib. I., Ch. VIII., Sec. 4). Thus Lord Kelvin's conceptions do not differ greatly from those of Democritus of Abdera, though the modern theories are much better established than the atomic theories were among the Greeks.

limit of resolution of the microscope which has been estimated by Michelson at 1/100,000 of an inch.

Now in our discussion of the constitution of granites we found that the *visible pores* in the quartz matrix have all diameters down to less than 1/10,000 of an inch, and thus practically to the lowest limit visible in the microscope. These visible pores thus evidently connect directly with the smaller invisible spaces which separate the molecules. As the diameters of the molecules in water vapor are only about 1/5,000 of the spaces between them, the triple atom of hydrogen and oxygen constituting water or water vapor would have ample facilities for penetrating a spongy and cavernous mass like granite with innumerable holes frequently of large size but always at least equal to the average free path. If the water or vapor were under pressure, so as to condense the fluid and thus increase the number of vibrations of a molecule per second, the rate of penetration of the fluid obviously would be much augmented.

And since granite not only is filled with pores of these various sizes, but also everywhere more or less cleft by planes of crystalline structures which are not really tight, but full of fissures and thus inviting the penetration of the fluid by the full power of capillary forces, we see that water would necessarily penetrate it at a fairly rapid rate. At the same time the influence of capillarity in such a structure is so great that although water might enter and slowly pass through it, even the development of steam pressure beneath the layer would not force the fluid back, because the steam pressure is nullified an infinitely small distance from where it is exerted, on account of capillary resistance; yet the fluid may keep on descending under the suction of the capillary forces so long as the supply from above is not cut off.

Upon these physical grounds it seems clear that there must be a secular leakage of the ocean bottoms, and a corresponding development of steam beneath the earth's crust. The steam expands the rock in which it is absorbed and in seeking release thus brings on earthquakes and mountain formation.

Even if the pressure due to depth should tighten up the structure of the rock in the lower layers of the crust, it would not be able to obliterate the leakage depending on the pores and crystalline

structure. It is evident that at depths such as twenty miles the downward movement of the fluid would continue, though very slowly. Hence the leakage of the oceans is extremely gradual, and the recurrence of earthquakes visibly delayed after relief has once been obtained. Thus while the tightness of the earth's crust due to the grain of the rock and the pressure to which it is subjected in the lower parts does not prevent ocean leakage, it makes the process so slow and gradual as to afford considerable protection to life upon our planet.

II. On the Physical State of the Earth's Interior, on the Average Rigidity of the Globe as a Whole, and on the Substratum of Plastic Matter Beneath the Crust which in Earthquakes Behaves as Fluid.

§ 12. *On the Theory of a Fluid Globe Held by the Older Geologists, and on Hopkins' Argument for Solidity Based on the Phenomena of Precession and Nutation.*—In the early part of the nineteenth century it was generally believed by geologists that the earth was a liquid globe covered by a rocky crust much thinner in proportion to the diameter than the shell is to that of an egg. This supposed liquid interior had been suggested by the streams of molten lava often observed to issue from volcanoes, and by the igneous rocks so abundantly poured forth in many places. The theory of a fluid globe seemed to be confirmed by the observed increase of temperature downward, which would give rise to molten rock at a depth of some twenty miles. The mountains and other phenomena traceable to dislocations of the crust could all be explained by a solid layer of this thickness, and the natural inference was that the great central nucleus remained liquid. The consolidation of the globe was ascribed to the progress of secular cooling, from the primitive state of high temperature assumed by Laplace in the nebular hypothesis postulated for explaining the origin of the solar system.

The older geologists had not adequately considered the effects of pressure in augmenting the solidity of the globe as we go downward; for since pressure raises the melting point of solids, the matter of the nucleus, though highly heated, might be solid if the

pressure be great enough to prevent fusion under the prevailing temperature. In order to throw light upon this question, Hopkins of Cambridge, England, took up the problem in 1839 (*Phil. Trans.*, 1839; "Researches in Physical Geology," 1839–1842), and sought to prove from the observed phenomena of precession and nutation that the earth could not be composed of a thin shell some twenty miles thick, filled with liquid. He concluded that the crust could not be less than 800 to 1,000 miles thick, and that the globe might even be solid to the center, except some small vesicular spaces here and there filled with molten rock.

In 1868 this subject was examined by the eminent French astronomer, Delaunay, who published a paper on "The Hypothesis of the Interior Fluidity of the Globe" (C. R. Acad. des Sci., Paris, July 13, 1868), in which he threw doubt on the views of Hopkins, and suggested that if the earth's nucleus were a mass of sufficient viscosity it might behave as if it were solid, and hence concluded that the observed phenomenon of precession and nutation did not necessarily exclude a fluid nucleus.

§ 13. *Lord Kelvin's Earliest Studies on the Precession of a Spheroid Containing Liquid.*—Lord Kelvin had already taken up the problem of the internal state of the earth in 1862, and considered the effects of a fluid nucleus enclosed in a thin shell when the whole mass was subjected to tidal strains. As the shell must yield under these strains the land would be carried up and down with the superjacent sea, and if such yielding occurred it ought to be sensible to observation. But since the sensible obliteration of the tides had not been observed, he naturally inclined to the view of Hopkins that the earth is effectively rigid and behaves as a solid globe.

In reply to Delaunay's criticism Lord Kelvin pointed out that if the French astronomer had worked out the problem mathematically he could not fail to see that the hypothesis of a viscous and quasi-rigid interior "breaks down when tested by a simple calculation of the amount of tangential force required to give to any globular portion of the interior mass the precessional and nutational motions which, with other physical astronomers, he attributes to the earth as a whole." (*Nature*, February 1, 1872.) On making this calculation Lord Kelvin found that the earth's crust down to depths of

hundreds of kilometers must be capable of resisting a tangential stress of nearly 0.1 of a gramme weight per square centimeter; this would rapidly draw out of shape any plastic substance which could be properly called a viscous fluid. "An angular distortion of 8″ is produced in a cube of glass by a distorting stress of about ten grammes weight per square centimeter. We may therefore safely conclude that the rigidity of the earth's interior or substance could not be less than a millionth of the rigidity of glass without very sensibly augmenting the lunar nineteen yearly nutation." (*Nature*, February 1, 1872, p. 258.)

Notwithstanding these early criticisms of Delaunay's paper, Lord Kelvin subsequently concluded that the phenomena of precession and nutation do not decisively settle the question of the earth's internal fluidity. Yet the semiannual and lunar fortnightly nutations may be considered to disprove absolutely the existence of a thin rigid shell full of liquid. If the fluid were arranged in successive layers of equal density, the only nutational or precessional influence exerted upon it would depend on the non-sphericity of the shells. "A very slight deviation of the inner surface of the shell from perfect sphericity would suffice," according to Lord Kelvin, "in virtue of the quasi-rigidity due to vortex motion, to hold back the shell from taking sensibly more precession than it would give to the liquid, and to cause the liquid (homogeneous or heterogeneous) and the shell to have sensibly the same precessional motion as if the whole constituted one rigid body." (Sir W. Thomson, British Assoc. Report, 1876, Sections, p. 5.)

It will be seen from this discussion that the argument from precession and nutation is only in part conclusive. If the fluid had a viscosity approaching high rigidity for rapidly acting forces, or it were subjected to such pressure that the particles in confinement acquired the properties of a solid, there would evidently be no sensible deviation from the precession and nutation appropriate to a cold solid globe.

§ 14. *On Lord Kelvin's Researches on the Earth's Rigidity Based on the Analysis of the Tides.*—The state of the earth's interior had early engaged the attention of Lord Kelvin, for the propagation of heat through the crust was before him as early as

1846. ("De Motu Caloris per Terræ Corpus," read before the faculty of the University of Glasgow in 1846; also a "Note on Certain Points in the Theory of Heat," February, 1844, published in the *Cambridge Mathematical Journal,* and reprinted in the "Mathematical and Physical Papers of Sir W. Thomson," 1882, Vol. I, Art. X.)

In a paper "On the Rigidity of the Earth" published in the *Philosophical Transactions of the Royal Society* for May, 1862, Lord Kelvin pointed out that if the matter of the earth's interior yielded readily to the tidal forces arising from the attraction of the sun and moon, the crust itself would respond to these forces in much the same way as the waters of the sea; and the corresponding movements of the crust would mask or largely reduce the height of the oceanic tides calculated for a rigid earth. By actual analysis of long series of tidal observations Kelvin and Darwin subsequently found the observed fortnightly tide to have very nearly its full theoretical height, and hence concluded that our globe as a whole possesses a very high effective rigidity. (Cf. Thomson and Tait's "Natural Philosophy," Vol. I, part II, § 832–847; also the article "Tides," Encyclopedia Britannica, ninth edition, § 44.)

Owing to the great importance of this work on the rigidity of the earth, we must trace the successive steps in the advancement of our knowledge. The assumption that the earth is made up of a liquid nucleus covered with a thin crust stiff enough to maintain its figure against the tide-raising forces of the sun and moon would imply that the crust has a degree of strength and rigidity not possessed by any known substance. It was therefore inferred by Lord Kelvin as early as 1862 that the crust might be 2,000 to 2,500 miles thick, in order to resist distortion under the tide-producing forces arising from the sun and moon.

"If the crust yielded *perfectly,* there would be no tides of the sea, no rising and falling relatively to the land, at all. The water would go up and down with the land, and there would be no relative movement; and in proportion as the crust is less or more rigid the tides would be more or less diminished in magnitude. Now we cannot consider the earth to be absolutely rigid and unyielding. No material that we know of is so. But I find from calculation that were the earth as a whole not more rigid than a similar globe of steel the relative rise and fall of the water in the tides would be only

two-thirds of that which it would be were the rigidity perfect; while, if the rigidity were no greater than that of a globe of glass, the relative rise and fall would be only two-fifth of that on a perfectly rigid globe.

"Imperfect as the comparison between theory and observation as to the actual height of the tides has been hitherto, it is scarcely possible to believe that the height is only two fifths of what it would be if, as has been universally assumed in tidal theories, the earth was perfectly rigid. It seems, therefore, nearly certain, with no other evidence than this afforded by the tides, that the tidal effective rigidity of the earth must be greater than that of glass. This is the result taking the earth as a globe uniformly rigid throughout. That a crust fifty or a hundred miles thick could possess such preternatural rigidity, as to give to the mass, part solid and part liquid, a rigidity as a whole, equal to that of glass or steel is incredible; and we are forced to the conclusion that the earth is not a mere thin shell filled with fluid, but is on the whole or in great part solid." (Paper read to Geological Society of Glasgow, February 14, 1878; Kelvin's "Popular Lectures and Addresses," Vol. II, pp. 317–318.)

In his presidential address to the Mathematical and Physical section of the British Association at Glasgow, September 7, 1876, Lord Kelvin remarked of the earth's crust that "were it of continuous steel and 500 kilometers thick, it would yield very nearly as much as if it were india rubber to the deforming influences of centrifugal force and of the sun's and moon's attractions." "The solid crust would yield so freely to the deforming influence of sun and moon that it would simply carry the waters of the ocean up and down with it, and there would be no sensible rise and fall of water relatively to the land." ("Popular Lectures," Vol. II., pp. 251–2.)

Lord Kelvin's final conclusion was that "the earth as a whole is certainly more rigid than glass, but perhaps not quite so rigid as steel."

§ 15. *Darwin's Researches on the Tidal Method of Evaluating the Earth's Rigidity.*—As the natural successor of Lord Kelvin in the researches on the physics of the earth, Professor Sir G. H. Darwin took up the problem of the earth's internal physical condition and confirmed and extended these conclusions by several important lines of inquiry. Darwin's researches on the bodily tides of viscous and semi-elastic spheroids and on the oceanic tides upon a yielding nucleus tended to strengthen the argument for a high effective rigidity so decidedly that he concluded that "no very considerable portion of the interior of the earth can even distantly approach the fluid condition."

But whilst Darwin's researches confirmed Kelvin's conclusions as to the great effective rigidity of the earth, yet a more critical

examination of the method for calculating the fortnightly tide led to the conviction that Laplace's argument is regard to the effects of friction was unsatisfactory. That friction would greatly effect the motion of the water in slow ocean currents within a few days was seen to be untenable. In consequence of this defect it turned out that long period tides as short as a fortnight would not enable the physicist to evaluate the rigidity of the earth, though the 18.6 yearly tide, depending on the revolution of the Moon's nodes, if it can be determined by observation, will eventually give the desired result. The height of this 18.6 yearly tide, however, is only one third of an inch at the equator, and great accuracy will be required for its detection.

Acting on the old belief Darwin compared the lunar fortnightly and monthly tides observed for 33 years at various Indian and European ports, with the equilibrium theory, and found that the tide-heights were about two thirds of the theoretical height. Accordingly he remarks: "On the whole we may fairly conclude that, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and the effective rigidity is at least as great as that of steel." (Thomson and Tait's "Nat. Phil.," Vol. 1, Part II, § 848.)

This was written prior to the discovery of the theoretical defect in the method of calculating the height of tides with periods not exceeding a fortnight in duration; yet even after the discovery of this defect it was still possible to infer that tides of long period in oceans such as ours must conform much more nearly to the equilibrium laws than do the tides of short period. "Whilst, then, this precise comparison with the rigidity of steel falls to the ground, the investigation remains as an important confirmation of Thomson's conclusion as to the great effective rigidity of the earth. . . . It appears by numerical calculation on viscous and elastico-viscous tides that in order that the oceanic semi-diurnal tide may have a value equal to two thirds of the full amount on a rigid globe, the stiffness of the globe must be about twenty thousand times as great as that of pitch at freezing temperature, when it is hard and brittle." (Sir G. H. Darwin, article "Tides," Ency. Brit., §§ 44–45.)

§ 16. *On the Rigidity of the Earth as found by Comparing the*

Observed Period of the Polar Motion Arising in the Variation of Latitude with the Theoretical Eulerian Period Calculated for a Rigid Earth.—The detection of the variation of latitude by Küstner at Berlin in 1890–91 and the subsequent discussion by Chandler of long series of observations showing that the movement of the pole in the body of the earth has a period of some 427 days, instead of the 305 days long ago inferred from Euler's theory of the rotation of a rigid spheroid, led Professor Newcomb to point out that this observed prolongation of the theoretical Eulerian period indicates some yielding of the matter of the globe under the stresses to which it is subjected by the movement of the pole, and would afford a new method of evaluating the earth's rigidity. In his well-known paper on the "Dynamics of the Earth's Rotation" (*Monthly Notices, R. A. S.*, March, 1892) Newcomb showed that the results already obtained decidedly confirmed Darwin's conclusion that the rigidity of the globe as a whole is comparable to that of steel.

The essential point in Newcomb's explanation is that when the pole changes its position in the body of the globe, the distribution of centrifugal force shifts with respect to the solid earth, which is thus put into a state of stress and must yield to the forces acting upon it, like any other elastic solid body; the periodic deformation of the earth's figure operating to lengthen the period of the free nutation, by an amount depending on the average rigidity of the whole earth.

The continued investigation of the variation of latitude carried out at the various international latitude observatories by Albrecht and others confirms this observational result, and the subject has also been examined theoretically by Darwin, Hough, Larmor and others; so that the validity of the method suggested by Newcomb is generally recognized.

In 1896 Mr. S. S. Hough treated of the problem in a very thorough manner in his well-known paper, "On the Rotation of an Elastic Spheroid" (Phil. Trans., A, 1896). He considered chiefly the case of an incompressible homogeneous spheroid, and was enabled to show by rigorous methods that the rigidity of the earth in all probability slightly exceeds that of steel.

In a remarkable paper "On the Period of the Earth's Free

Eulerian Precession," read to the Cambridge Philosophical Society, May 25, 1896, Professor Larmor showed how to estimate the effect of the elastic yielding of a rotating solid on the period and character of the free precession of its axis of rotation, and again confirmed the high effective rigidity of the earth from another point of view.

The observed prolongation of the Eulerian period is thus fully explained by the imperfect rigidity of the earth's mass, and the high rigidity thus deduced has naturally strengthened the earlier conclusions of Kelvin and Darwin drawn from the study of the long period tides of the sea.

This investigation, like those already cited, gives us only an average effect for the earth as a whole, but does not tell us the law of the distribution of rigidity within the globe. If this law of distribution of rigidity could be found, even approximately, it would be of great interest, because we could then see in what part of the globe the principal part of the yielding takes place; and this would give us a much better understanding of the internal constitution of our planet than heretofore has been considered possible.

§ 17. *Rigidity of the Earth Calculated from the Theory of Gravity, on the Hypothesis that the Distribution of Rigidity in the Globe is Everywhere Proportional to the Pressure.*—It has not been supposed by previous investigators that a method could be devised for deducing the rigidity of a body like the earth from the theory of gravity; but in 1905 it occurred to the present writer that such a method could be found if we could adopt a suitable hypothesis for the variation of the rigidity with the pressure. Previous investigations of the internal state of the heavenly bodies had justified the law of Laplace as giving an excellent approximation to the law of density for the earth and the rest of the encrusted planets; and the monatomic law had been found most satisfactory for the sun and fixed stars (cf. *A. N.*, 4053). These laws enable one to obtain the pressure at every point of the radius of the heavenly bodies. For in several ways Laplace's law of density is fairly well established for the earth, and on equally good grounds the density of the sun is believed to conform essentially to the monatomic law.

From a study of the laws of density, pressure and temperature within the heavenly bodies it appeared to me (as it had indepen-

dently appeared to Arrhenius five years before) that matter under these extreme conditions must be essentially gaseous; and as it is above the critical temperature, it is made to behave in confinement as an elastic solid. Now in all gaseous masses the density is proportional to the pressure so long as the gas remains perfect; and the gas does not cease to be perfect when the temperature is above the critical value, though it may acquire in confinement the property of an elastic solid if the pressure be great enough to bring the molecules within a distance at which the molecular forces become effective in spite of the high temperature. Thus while the property of rigidity in cold solids depends wholly on molecular forces which prevent deformation, this property for gaseous matter in confinement under such pressure that it acquires the property of an elastic solid, is due wholly to the pressure. The molecular forces giving effective rigidity must increase in proportion to the pressure, or in a higher ratio.

If according to hypothesis the matter is made solid by pressure, then the molecular forces resisting deformation in the imprisoned matter thus solidified cannot resist deformation in a less degree than the direct proportion to the pressure on which the solidification depends. And any ratio higher than the direct proportionality to the pressure would most likely depend on the temperature. Now the temperature in the earth is supposed to be everywhere such as to make the density conform essentially to Laplace's law; and the pressure resulting from this law of density gives the matter everywhere the property of an elastic solid, and therefore its molecular properties must correspond to the physical state determined by the laws of density and pressure.

It is of course conceivable that some parts of the globe might be relatively more rigid than is required to give solidity, but the effect of this would only increase the average rigidity of the earth as a whole. And since seismological and other observations seem to show that the globe is solid throughout, except a thin layer just beneath the crust, the hypothesis of a rigidity proportional to the pressure will give a true minimum value of the earth's rigidity.

Now on the hypothesis that the density follows Laplace's law,

the pressure throughout the earth's mass is given by the formula (cf. *A. N.*, 4104)

$$p = \frac{3x}{2(\sigma_1 g)q^2}[(\sigma g)^2 - (\delta g)^2], \tag{1}$$

where r is the radius of the earth, g mean gravity, q the constant for Laplace's law, 2.52896 radians $=$ 144° 53′ 55″.2, σ the density at any point, δ the density at the surface, and σ_1 the mean density.

To render this expression available for integration throughout the sphere occupied by the earth's mass, we must put for σ^2 its value

$$\sigma^2 = \sigma_0^2 \frac{\sin^2(qx)}{q^2x^2},$$

and for δ^2 its value

$$\delta^2 = \sigma_0^2 \frac{\sin^2 q}{q^2},$$

corresponding to the surface where $x = 1$. Thus we obtain

$$p = \frac{3(\sigma_0 g)^2 r}{2(\sigma_1 g)q^2}\left[\frac{\sin^2(qx)}{q^2x^2} - \frac{\sin^2 q}{q^2}\right]. \tag{2}$$

For the total pressure throughout a sphere of radius $\rho = rx$, r being the external radius, and $x = (\rho/r) =$ fraction of the radius, we have

$$\begin{aligned} P &= \int_0^x p \cdot 4\pi r^2 x^2 \cdot r dx \\ &= \frac{3(\sigma_0 g)^2 \cdot r 4\pi r^3}{2(\sigma_1 g)q^4}\left(\int_0^x \frac{\sin^2(qx)}{x^2} x^2 dx - \sin^2 q \int_0^x x^2 dx\right), \end{aligned} \tag{3}$$

which by integration becomes

$$P = \frac{3(\sigma_0 g)^2 r 4\pi r^3}{2(\sigma_1 g)q^4}\left(\frac{qx - \sin(qx)\cos(qx)}{2q} - \sin^2 q \frac{x^3}{3}\right). \tag{4}$$

As our integration is to include the whole sphere of the earth, we put $x = 1$, and then we have

$$P = \frac{3(\sigma_0 g)^2 \cdot r 4\pi r^3}{2(\sigma_1 g)q^4}\left(\frac{q - \sin q \cos q}{2q} - \frac{\sin^2 q}{3}\right). \tag{5}$$

The total volume of the earth is $(4/3)\pi r^3$, and hence the average pressure per unit of area on all concentric spherical surfaces is

$$R = \frac{P}{\frac{4}{3}\pi r^3} = \frac{3}{4\pi r^3}\int_0^x p \cdot 4\pi r^2 x^2 \cdot r\,dx$$
$$= \frac{9(\sigma_0 g)^2 \cdot r}{2(\sigma_1 g)q^4}\left(\frac{q - \sin q \cos q}{2q} - \frac{\sin^2 q}{3}\right). \quad (6)$$

If r is expressed in meters, the mean pressure or mean rigidity R comes out in kilograms per square meter. To reduce the result to atmospheres we divide by 10,333. The result for the earth is $R = 748{,}843$ atmospheres, about the rigidity of wrought iron.

This method takes no account of the earth's solid crust, and is therefore too small; moreover viscosity increases within the earth, owing to the rise of temperature downward. We give hereafter an approximation to the increase of rigidity by determining the mean rigidity of the earth's *matter,* as distinguished from that of the *various layers* composing the globe, just found by the above analysis.

To find the mean rigidity of the earth's *matter* we must consider not only the pressure but also the density or mass per unit volume of the imprisoned matter in each layer. The result represents a mean rigidity in which every elementary spherical shell composing the globe is allowed a weight proportional to its mass, which is multiplied by the pressure to which it is subjected.

The theory of the determination of the mean rigidity of the earth's matter is as follows:

$$P' = \int_0^x p \cdot 4\pi r^2 x^2 \cdot r\,dx \cdot \sigma = 4\pi r^3 \sigma_0 \int_0^x p \cdot x^2 dx \frac{\sin(qx)}{qx}. \quad (7)$$

Substituting for p its value from (2), we get

$$P' = \frac{3(\sigma_0 g)^2 \cdot r \cdot 4\pi r^3 \sigma_0}{2(\sigma_1 g)q^5}\left(\int_0^x \frac{\sin^3(qx)x^2 dx}{x^3} - \sin^2 q \int_0^x \frac{x^2 \sin(qx)dx}{x}\right), \quad (8)$$

$$= \frac{3(\sigma_0 g)^2 \cdot r \cdot 4\pi r^3 \cdot \sigma_0}{2(\sigma_1 g)q^5}\left(\int_0^x \frac{\sin^3(qx)qdx}{qx} - \sin^2 q \int_0^x \frac{x \sin(qx)qdx}{q}\right). \quad (9)$$

The integral of this last term is $-\sin^2 q\ \dfrac{\sin(qx) - qx\cos(qx)}{q^2}$.

The value of the first integral is most conveniently found by quadrature, table for which is given in *A. N.*, 4104, p. 379. Dividing out the mass, or volume of the sphere by the density, we have

$$R' = \frac{P'}{\frac{4}{3}\pi\sigma_1 r^3} = \frac{3\int_0^x p \cdot 4\pi r^2 x^2 \cdot r dx \cdot \sigma}{4\pi\sigma_1 r^3} \qquad (10)$$

$$= \frac{9(\sigma_0 g)^2 \cdot r \cdot \sigma_0}{2(\sigma_1 g) q^5 \sigma_1}\left(\int_0^x \frac{\sin^3(qx) q dx}{qx} - \frac{\sin^2 q}{q^2}[\sin(qx) - qx\cos(qx)]\right).$$

On putting $qx = 144° 53' 55''.2$, the value of the integral is found by quadrature to be 0.9592502, and when the rest of the formula is reduced to numbers we have (*A. N.*, 4104):

$$R' = 1028702 \text{ atmospheres.}$$

The rigidity of nickel steel is taken to be 1,000,000 atmospheres. It thus appears from this calculation that the average rigidity of all the earth's matter somewhat exceeds that of nickel steel. The actual rigidity of the earth almost certainly lies between the limits thus established, namely $R = 748{,}843$, based on the rigidity of the layers deduced from the pressure to which they are subjected, and $R' = 1{,}028{,}702$, derived from the product of the mass of each layer by the pressure acting upon it.

In the paper, "Researches on the Rigidity of the Heavenly Bodies," *A. N.*, 4104, the rigidity of the earth is discussed as follows:

"When one considers the effects of the enclosing crust and the viscosity of the whole earth, which must be assumed to increase towards the centre, owing to the increasing density and rising temperature of the imprisoned matter, it seems not improbable that the actual effective rigidity of our globe may be nearer the upper limit than the lower, and probably we shall not be far wrong in concluding that it is approximately equal to that of nickel steel.

"Leaving aside the consideration of the effects of the solidified crust, it is evident from the nature of the forces at work that most of the yielding of our globe, due to the periodic action of small forces, is in the outer layers; and in general the yielding in any concentric layer may be taken to be inversely as the pressure to which the imprisoned matter is subjected. *It is remarkable that the curve of pressure as we descend in the earth becomes therefore also the curve of effective rigidity for the matter of which the earth is composed.* Thus the rigidity of the matter at the earth's center probably is at least three times that of nickel steel used in armor plate; as we approach the surface the effective rigidity constantly exceeds that of nickel steel until we come within less than 0.4 of the radius from the surface, where the pressure is less than 1,000,000 atmospheres.

"To imagine a mechanical substitute for the earth's constitution, without the introduction of pressure, suppose an alloy of adamant to give the material at the centre of such a globe, of the same size but devoid of gravitation, a hardness three times that of armor plate. The outer layers as we approach the surface must then be supposed softer and softer, until it is like armor plate at a little over 0.6 from the center, and finally a very stiff fluid near the surface. In addition to this arrangement of its effective internal rigidity the actual earth is enclosed in a spheroidal shell of solid rock analogous to granite. One can easily see that tidal forces applied to all the particles of such an artificial armored sphere would produce but very slight deformation, because of the enormous effective rigidity of the nucleus.

"The principal uncertainty in this result arises from the admissible variations in the assumed Laplacean distribution of density within the earth. Both Radau and Darwin (cf. *Monthly Notices*, Roy. Astron. Soc., December, 1899) have pointed out that considerable variations in the internal distribution of density are possible without invalidating the well-known argument drawn from the phenomenon of the precession of the equinoxes; yet on physical grounds it seems clear that pressure is the principal cause of the increase of density towards the earth's centre. And since this does not vary greatly for moderate changes in the law of density, the principle of continuity shows that the actual law of density within the earth cannot depart very widely from that of Laplace. The above value of the theoretical rigidity of the earth may therefore be taken as essentially accurate, and I think no doubt can remain that the rigidity of our earth as a whole considerably exceeds that of steel. The original conclusions of Kelvin and Darwin are therefore confirmed by the present dynamical considerations based upon the theory of universal gravitation."

In this connection we should remember that the experimental rigidity of steel is 808,000 and of glass 235,000 atmospheres. The calculated rigidity of all the matter within the globe, found by considering not only all the layers, but also the density in each layer, is found to be 1,028,702 atmospheres. Now the average rigidity must be greater than 750,000, because the stiffness of the crust and increase of viscosity downward is neglected in the gravitational method. In fact this method is not applicable to the outermost layers, because the pressure there is much less than the rigidity, and only becomes equal to the rigidity at a depth of something like one tenth of the radius, where the pressure is 320,295 atmospheres.

According to the experiments of Milne and Gray the rigidity of granite is about one sixth that of steel; and as steel has a rigidity of 808,000 atmospheres, that of granite is about 135,000 atmospheres, or a little more than one half that of glass. We may therefore take

the outer layers of our globe to have a rigidity about half that of glass, and assume that at a depth of 0.1 of the radius it becomes nearly 2.5 times as great as it is at the surface.

Whether it becomes at a depth of twenty miles less than it is at the surface we cannot tell, but such a decrease is not impossible, perhaps not improbable; because at this depth the molten rock moves in earthquakes, and yet in confinement it must have a very sensible rigidity, though probably not more than half that of granite.

Accordingly, it looks as if the rigidity at the surface is about half that of glass, at a depth of 20 miles about one half that at the surface, and at the depth of 40 miles nearly the same, but increasing below that depth and at 160 miles again equal to that at the surface, and at a depth of 400 miles considerably larger yet, or about 1.4 times that of glass. Increasing below this depth according to the pressure, it becomes at the center over 3 times that of nickel steel used in armor plate. The rigidity of steel is attained at a little over 0.3 of the depth to the center of the earth. If this be the distribution of rigidity in the earth, the curve of rigidity is as follows:

This postulated fall in the rigidity just beneath the crust is probable for several reasons:

1. The temperature increases quite rapidly as we go downward, while the pressure increases proportionately more slowly, so that a depth would be reached at which the matter would become a plastic if not a viscous fluid.

2. The eruption of volcanoes and lava flows on a vaster scale show that a molten layer underlies the crust, and occasionally is forced to the surface.

3. This underlying molten rock moves in world-shaking earthquakes, and frequently is expelled from beneath the sea under the land to form mountain ranges along the coast.

4. We may prove this expulsion of lava by the observed seismic sea waves which indicate a sinking of the sea bottom, and by the simultaneous uplift of mountains and coasts.

From these considerations it follows that the earth is most nearly liquid just beneath the crust, and has the greatest rigidity at the center. As the plastic or quasi-viscous layer beneath the crust is thin, and possessed of considerable rigidity, *it too remains quiescent*

except when set in motion by the dreadful paroxysms of an earthquake.

In tidal and other observations the earth therefore behaves as a solid, and the rigidity of the earth inferred by Kelvin and Darwin is confirmed. Yet a layer of plastic matter or quasi-viscous fluid exists just beneath the crust, and when disturbed by earthquakes gives rise to the development of ridges in the crust called mountains, chiefly by the expulsion of lava from under the sea.

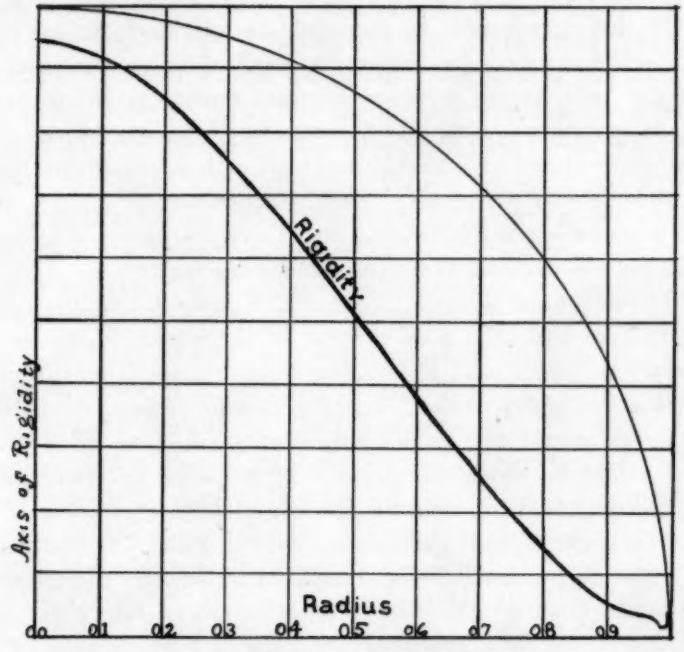


FIG. 2. Curve of Rigidity for the Earth, showing the plastic layer just beneath the crust.

§ 18. *Wiechert's Researches on the Interior Constitution of the Earth and on the Plastic or Viscous Layer which he Infers to Exist Just Beneath the Crust from Oscillations of Long Period Noticed in Seismic Vibrations.*—Professor E. Wiechert, of Göttingen, has devoted much attention to the problem of the constitution of the earth's interior. He long ago reached the conclusion that the great interior nucleus probably is a mass of iron covered with a thick

shell of stony material. In the paper which he recently presented to the International Seismological Association in session at the Hague, September 21–26, 1907, he estimates the depth of the stony layer as 1,500 kilometers, which is nearly one fourth of the earth's radius.

This view that interior of the earth is metallic has been entertained by many eminent physicists, including Lord Rayleigh; but it is beset with many difficulties. We shall here mention three of the principal objections:

1. If this constitution of the earth be admitted, the curve of density will have a sudden break at a depth of about one fourth of the radius; and, as the pressure increases rapidly as we go downward, it seems improbable that the density of the outer layer could remain uniform and then change suddenly at a depth of one fourth of the distance to the center. Such discontinuity in nature seems highly improbable for the density, since there probably is no sensible discontinuity in the laws of pressure and temperature.

2. If the central nucleus is metallic, it follows that the denser elements have separated from the rest of the mass. As the matter has been essentially solid and highly rigid, owing to the pressure, ever since the globe attained anything like its present dimensions, this sinking would not be possible, because the resistance to the motion would be much too great. Thus owing to resistance to motion arising from rigidity we can not admit a separation of the denser from the lighter elements of such a globe. If the metals were all so deep down, it would be hard to account for the veins found in the crust by any kind of eruptive process, since the globe is never fissured to a depth of anything like one fourth of the radius.

3. If in addition to these mechanical objections we recall that deep down the pressure is so great as to cause an interpenetration of all the elements, whatever be the temperature, but especially under the high temperature known to prevail in the interior of the globe, so that no aggregation or crystallization of substances would be possible, and the nucleus would therefore be a magma of all the elements, it becomes inconceivable that the metals could separate from the stony elements by sinking, while the latter floated to the

surface. Even if the globe were a liquid mass of very small viscosity, it is clear that such a separation of the elements could not take place.

Finally it is to be recalled that recent experiments with radium have shown the probable transmutation of some of the metals, as when Sir Wm. Ramsay caused sulphate of copper to be partially degraded into lithium. If this can occur for one or two metallic elements, it may eventually be possible for many and perhaps all of the metals. Our knowledge of these transformations is still in its infancy, and we can not yet ascertain how minerals and metallic veins have arisen; but it is impossible to believe that the material has come up from a pure supply at a depth of 1,500 kilometers. It is much more probable that the metallic elements have been developed by differentiation and transformation from an original magma, and that the whole interior of our planet is still a magma. Differentiation of the elements appears to develop under conditions met with in the crust, but nowhere else.

Accordingly we are obliged to dissent from the constitution of the globe outlined by Professor Wiechert; but in the matter of the existence of a layer of plastic or fluid material just beneath the crust, which he infers from the long seismic vibrations with periods of about eighteen seconds, we are in hearty accord with him. This is definitely proved by the phenomena noticed in earthquakes, as more fully set forth hereafter. It is the expulsion of lava from under the margins of the sea which produces world-shaking earthquakes and the upheaval of mountains along the sea coasts.

§ 19. *On Sir G. H. Darwin's Researches on the Stresses in the Interior of the Earth Due to the Weight of Continents and Mountains.*—We have seen that the earth behaves as a solid at all depths, unless it is in the thin layer just beneath the crust, in which movements take place during earthquakes. The theory of an elastic solid shows that when such a body is stressed the state of stress is completely determined when the amount and direction of the three principal stresses are known. No limit is imposed on these stresses by theory, but in practice nature fixes a limit, beyond which the elasticity breaks down, and the solid either flows or ruptures by breaking.

In the "Nat. Phil.," Vol. I, part II, § 832, Lord Kelvin and Professor Tait remark that

> "The precise circumstances under which elastic bodies break have not hitherto been adequately investigated by experiment. It seems certain that rupture cannot take place without difference of stress in different directions. One essential element therefore is the difference between the greatest and least of the three principal stresses. How much the tendency to break is influenced by the amount of the intermediate principal stress is quite unknown. The difference between the greatest and least stresses may however be taken as the most important datum for estimating the tendency to break. This difference has been called by Mr. G. H. Darwin (to whom the investigation of which we speak is due) the 'stress-difference.'"

Stress-difference is a term which when applied to matter within the earth denotes the tendency to flow. For rupture is not possible when the matter is in confinement under such pressure and at high temperature. Now if the earth were homogeneous, as assumed in Darwin's inquiry, the inequalities of surface due to the mountains, plateaus, and continents would give rise to a stress-difference in the underlying layers; and Darwin showed that the stress-difference would increase with the depth, being at the center, for inequalities of the type represented by harmonics of the second order, eight times what it is at the surface.

If the earth were not effectively solid throughout, a flow ought to take place either near the surface or at greater depth; and thus the inequalities of surface would disappear. But the plateaus and mountains do not sink in, and this fact proves that the globe is not fluid, and even that the plastic or viscous layer just beneath the crust is quite stiff. As we have seen that the rigidity increases very rapidly towards the center, we easily see why movement should not occur at great depth, since the rigidity there exceeds that of any known substance, and at the centre is about three times that nickel steel used in armor plate.

In the paper on the "Temperature of the Earth" we have shown from the evidence of stability afforded by geological pinnacles millions of years old, that no movements of deep seated character occur within the earth. This evidence supports the view that the earth is effectively solid, and has behaved as such since the consolidation of the crust.

As the rigidity increases so rapidly towards the center of the earth, flow ought not to take place at those depths; and the absence of any evidence of deep seated movements among the ruins wrought by geological time in turn supports the theory of rigidity depending on the pressure.

Darwin's hypothesis of homogeneity is only a rough approximation to the truth, and Laplace's law would no doubt give a much more exact representation of the density and the resulting stress-difference in the earth. But this suggested change of data would not greatly modify the general conclusions already stated.

§ 20. *The Theory of Isostacy.*—A more important difference might arise from the theory of isostacy, the applicability of which to the earth seems to be becoming better established by recent researches. In this view the crustal inequalities seen at the surface are compensated for by lighter or greater densities beneath, according as the crust is elevated or depressed, so that for a certain thickness of crust equal blocks have equal mass, however unequal the level of the blocks at the surface.

The recent investigations by the U. S. Coast Survey indicate that the depth of complete compensation for the United States and outlying stations is about 71 miles. No doubt a depth of something like this extent would hold true for the entire globe. If this view be admissible, it will follow that all inequalities of the crust cease to be effective at depths greater than 71 miles, and no stress-differences depending on plateaus and mountains would exist in the globe except in the layers just beneath the crust. There would thus be no stresses in the deep interior depending on the weight of continents and mountains.

This theory of isostacy is confirmed by the theory of mountain formation developed in the paper on the "Cause of Earthquakes," which shows clearly that these elevated ridges are underlaid by material lighter than the average rock of the crust. On the one hand, therefore, if stress-differences exist deep down, no movement can take place, owing to rigidity; on the other, if the theory of isostacy be admissible, no stress-differences can exist except in the outer layers of the globe, within 71 miles of the surface.

We conclude therefore that in no case could movements occur

except in the layer just beneath the crust. These superficial movements are called earthquakes, and are caused chiefly by the leakage of the oceans. Observations show that the depth of such disturbances in all cases is less than 40 miles. This accords with the theory of isostacy, and confirms the conclusions drawn from that theory that all surface inequalities are compensated for at but a slight depth.

§ 21. *Uplifts along the Andes show that the mountains are not sinking under their own weight.*—In Professor Sir G. H. Darwin's paper on the stresses in the earth, above cited, he has also considered harmonics of high order, corresponding to the case of a series of parallel mountains and valleys, which thus corrugate a mean level surface with an infinite series of parallel ridges and furrows. Here the stress-difference depends only on the depth below the surface, and is independent of the position of the point considered with respect to ridge and furrow. Taking a series of mountains 13,000 feet (about 4,000 meters) above the valley bottoms, formed of granite of density 2.8, he shows that the maximum stress-difference is 4×10^5 grammes weight per square centimeter (about the tenacity of cast tin). And when the mountain chains are 314 kilometers apart, making the ridges about 78 times wider than they are deep, the maximum stress-difference is reached at a depth of 50 kilometers below the surface, or at a depth of 12½ times the height of the mountains above the valleys. Thus for mountains of the height of our average ocean depth, the maximum tendency to flow would be at a depth of about 31 miles. (Cf. "Nat. Phil.," Vol. I, Part II, § 832.)

If earthquake shocks were due to such flowage the mountains would be gradually reduced in height. Instead of this settling occurring, mountains like the Andes are still rising, as we may infer from the fact that after an earthquake the adjacent sea coast often is elevated and higher than before; while the sinking of the adjacent sea bottom, indicated by the accompanying seismic sea wave, shows that the bed of the sea was undermined by the expulsion of the material pushed under the land and mountains. *This state of fact emphatically contradicts the view that these great seismic disturbances are due to the flowage beneath the crust arising from the*

weight of continents and mountains. Neither the uplift of mountains about the sea coasts, nor the earthquakes occurring in these regions can be explained by flowage beneath the crust, because the movement is positive rather than negative, as required by this theory.

Whilst the investigation of Professor Sir George Darwin therefore does not give us a clue to the observed movements, it is nevertheless very valuable as furnishing an indirect confirmation of the present theory that mountain formation depends on the sea. Observation shows that the movements are positive, and as the theory of flowage indicates that they should be negative, we may infer that whatever be the stress-differences existing beneath the earth's crust, the movements thus produced are insensible compared to those depending on the expulsion of lava from under the sea by world-shaking earthquakes.

III. The New Physical Theory of Earthquakes and Mountain Formation Based on the Secular Leakage of the Ocean Bottoms.

§ 22. *On the Plastic and Perhaps Viscous Layers Just Beneath the Earth's Crust.*—We have now examined at length the arguments in regard to the constitution of the earth's interior, and have shown that although as a whole the earth is solid, owing to the pressure to which the matter is subjected, there is a plastic layer just beneath the crust which in earthquakes is made to flow and behave almost as a viscous fluid. In this layer just beneath the crust either the pressure is not great enough to produce entire solidity, with the existing temperature, or else the solid is made to flow by the breaking down of the elasticity under the action of the earthquake forces, which are powerful enough to disturb the whole world.

Although the matter in this substratum appears to have some rigidity, it seems probable that it has not the requisite elasticity to behave as a perfect solid. We know that the layer must be nearly solid, because, if it were not so, there would be a greater tendency of the mountains to subside than actually is observed. The stress-difference in the layers just beneath the crust must be very considerable; and yet this plastic matter is so stiff that it does not flow and allow the mountains and plateaus to sink in.

Now earthquake disturbances are often complex, and consist in horizontal and vertical movements combined. We have seen that in the long run the uplifting tendency predominates, because it is in this way that the mountains and plateaus have arisen. Nevertheless there are numerous cases in which subsidences take place, and these settlements often seem to be somewhat gradual, as if the substratum was slowly yielding and flowing under the stresses to which it is subjected. These gradual subsidences, of the class that was observed by Darwin and Fitzroy at Conception in 1835, seem to afford convincing evidence that the layer beneath the crust is certainly plastic, perhaps viscous.[4] The yielding of the layer beneath the crust is shown not only in movements noticed in earthquakes, when lava is expelled from under the sea and pushed under the land; but also in the subsidences which the sea trenches experience after earthquakes. These subsidences have folded the rocks seen in mountain ranges now on land; and although most of such subsidence is due to the undermining of the troughs by the expulsion of lava, it seems likely that some very gradual yielding also takes place. The layer under the crust is therefore certainly plastic, when partially undermined, and probably so, independent of the undermining, if it is subjected to great forces, as in world-shaking earthquakes, where mountains are in process of upheaval. If the matter is also viscous, the viscosity must be very high. With the matter imprisoned beneath the earth's crust it is difficult if not impossible to distinguish between plasticity and true viscosity, because, if the fluid is very stiff, it would behave almost as a solid. And the tests heretofore afforded by earthquakes are not decisive. This view of the substratum just beneath the crust is not essentially different from the theory held by Arrhenius with regard to the interior of the earth as a whole. But this layer is the only part of the interior in which movements may be observed, and even here movements would not take place but for the steam developed beneath the crust by the secular leakage of the oceans. It may be that the future study of these movements will some day

[4] We follow Sir George Darwin in "distinguishing viscosity, in which flow is caused by infinitesimal forces, from plasticity in which permanent distortion or flow sets in when the stresses exceed a certain limit." (Letter to Sir A. Geikie, January 9, 1884.)

enable us to decide whether the substratum is plastic only, or truly viscous.

§ 23. *Substratum Everywhere Quiescent Except when Disturbed by Earthquakes.*—The fact the large areas of the earth's surface in such dry countries as Sahara, our Western Plateaus, and the interior of Australia, are quite free from earthquake disturbances, shows what would happen everywhere but for the presence of surface water, and especially the leakage of the crust depending on the sea. The quiescence of the substratum in interior regions remote from the sea shows that under normal conditions this layer is quite inert. It is only set in motion by the vapor of steam which slowly develops stresses in the rocks of the crust and finally brings on earthquakes. It might be plastic enough to yield slightly under sufficiently great forces, but the loading and unloading due to meteorological and geological causes going on in nature are not great enough to have any appreciable effect, as we may infer from the universal quiescence of inland areas, especially in desert countries.

It seems to be true, however, that when the crust is broken and upheaved, in the formation of mountains near the sea coast, some slow yielding takes place beneath. Yet at present any changes of a creeping nature can not be entirely separated from those depending on the expansion and expulsion of lava from under the sea; and we can only feel sure of the inert character of the substratum, except where disturbed by water vapor entering from without. Along the sea coasts the stresses in the crust are constantly changing, and the crust blocks yielding more or less to the stresses acting upon them; it is only when *sudden yielding* occurs that we experience a shock, and the greatest earthquakes are characterized by molten rock adjusting itself beneath the crust. It is probable that much yielding takes place which is exceedingly gradual and produces no disturbances sensible to ordinary observation. In dry regions remote from the sea there are no shocks, and therefore also no gradual yielding of the crust; hence the substratum is inherently and naturally quiescent except when disturbed by external forces.

§ 24. *Mountain Formation in the Sea and on the Land.*—In the paper on the "New Theory of Earthquakes and Mountain Formation," we have cited certain cases of mountain formation now going

FIG. 3. Relief Map of North America. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) This map illustrates beautifully the recession of the sea since the formation of the Rocky Mountains, which were at one time the eastern border of the Pacific Ocean.

on in the depths of the sea, and directly connected with mountain systems spread out on the land. Thus we have shown that the Aleutian Islands are a branch or part of the Rocky Mountains still remaining in the depths of the sea. As this part of the chain is now being uplifted by the ocean, we get a very clear conception of how the whole Rocky Mountain system was formed. We are fortunate therefore to find a part of a great mountain chain still unfinished, with one end under water and the main body of the system high and dry along the edge of the continent.

Now no one believes[1] that mountain formation takes place far inland, because the mountains generally follow the coast, and moreover at present the process is found to be most active *in the sea,* as in the region of the Aleutian Islands and the Antandes. This geographical distribution of mountain-making is therefore a most powerful argument for the new theory. Moreover it is generally recognized that the Rocky Mountains in the United States are a good deal older than the Andes in South America; and as the relative ages bear some relation to the distances from the sea, the mountains on land give the same indication as those still in the depths of the sea. The recession of the sea goes on at very unequal rates in different parts of the world, yet the present positions of the mountains show that the older mountains are generally remote from the ocean. The present theory is therefore confirmed by the lay of the older as well as of the younger mountain systems; and by the situation of the mountains on land as well as of those now being formed in the depths of the sea. All the mountain phenomena of the globe are thus shown to be consistent. But as direct observation of mountain formation witnessed with our own eyes is the most convincing of all evidence, it is fortunate that we are able to cite numerous cases of mountain ranges now developing in the sea. By the study of the sinking going on where trenches are developing, we see how the wrinkles and valleys were produced in mountain systems now at a considerable distance from the ocean. Since the sea recedes from the mountains in the course of geological ages, it follows that more and more land is constantly rising above the water,

[1] Compare § 42 of this paper, where Leconte's views are quoted at length. He held that mountain ranges are formed on lines of thick sediment along the shores of continents.

and the continents growing larger. The mountains are formed by earthquakes, and earthquakes are due to the sea, which thus makes more and more land for the development of the higher forms of life upon the globe.

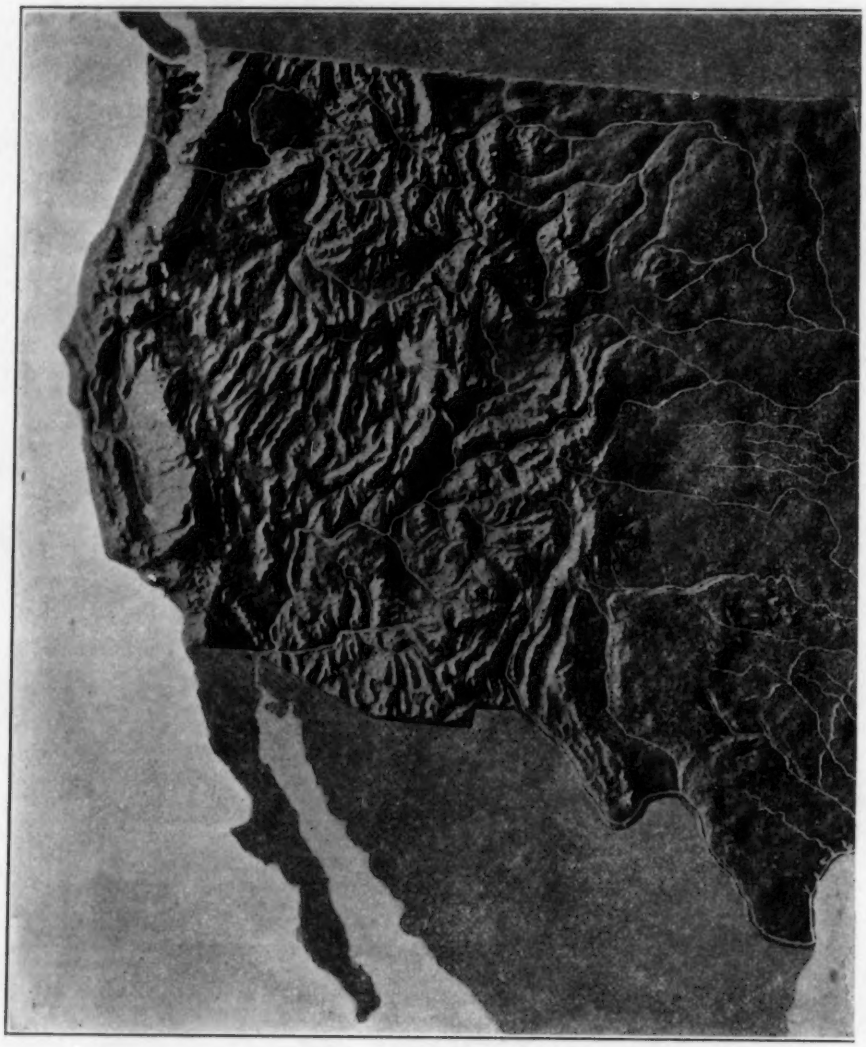


FIG. 4. Relief Map of the United States. (From Frye's Complete Geog-
especially how the great plateau west of the Rocky Mountains has been

§ 25. *The Origin of Faults in the Earth's Crust.*—It has long been recognized that faults in the earth's crust are often displaced by earthquakes. Now earthquakes are mainly submarine or follow the borders of the continents. Here the mountain ranges have de-

raphy, by permission of Ginn & Co., Publishers.) The reader should notice crumpled in the uplift from the sea, which has receded westward 1,000 miles.

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veloped or are now developing, and in general the faults run along the sea coasts and into the sea, where mountain formation is in progress. Thus it is clear that faults arise from the stresses and movements of the crust produced by earthquakes and mountain formation, and therefore from the secular leakage of the ocean bottoms.

Sometimes the faults move but little, at other times they give rise to conspicuous changes of level; and where vast down-throws or uplifts have occurred certain types of mountains arise from normal faulting. The more horizontal movements of faults arise mainly in the trenches along the sea coasts, which produce the folding seen in mountain chains. The vertical movements are more general, and are especially conspicuous in elevated plateaus, like those of our western states.

In his "Report on the Geology of the High Plateaus of Utah," Washington, 1880, Major Dutton gives a description of some of the most magnificent faults in the world. On page 45 he indicates the dependence of these faults on the ancient shore line of the Eocene lake, thus:

> "It yet remains to speak of another interesting relation of the later system of faults. They have throughout preserved a remarkable and persistent parallelism to the old shore line of the Eocene lake, following the broader features of its trend in a striking manner. The cause of this relation is to me quite inexplicable, so much so, that I am utterly at a loss to think of any subsidiary facts which may be mentioned in connection with it and which can throw light upon it."

What puzzled Major Dutton most was the raising of the area of the lake; but as the whole region was uplifted by the sea in later times this phenomenon was in no way remarkable. The rocks in such disturbed regions have been broken and folded into a series of troughs and arches or thrown into domes and basins, and probably no two adjacent areas retained their relative levels throughout. His observation, however, confirms the present theory that faulting is generally parallel to the ancient sea shore, and therefore produced originally by the oceans.

The conspicuous character of the vertical movement of the crust blocks in the region of the Great Basin led several American geologists to suggest that vertical forces had operated in the uplift of

these plateaus. As the whole region has been raised from the sea by the injection of the land with lava pushed under the crust from beneath the sea, it is evident that the crust blocks ought to be displaced unequally in different places, and hence the various types of faulting observed.

It should be remarked, however, that in the elevation of a plateau a mile high, only a layer of lava a mile deep needs to be injected. If three miles high, the layer would have to be three miles thick; but even this maximum height is only about one seventh of the thickness of the crust; and hence eruptions would not usually occur in these uplifts. The plateaus are all of small height compared to the thickness of the earth's crust, beneath which the movement of molten rock takes place.

If some faults should thus be widely opened, lava flows of vast extent, like those in Utah and Oregon, might be expected to occur. We cannot give the details of the cracks which produced these gigantic outflows, but it is evident that they depended on the opening of immense faults. Now the faults are produced and moved by earthquakes, and earthquakes are due to the leakage of the oceans. It follows therefore that the most immense lava flows ought to take place near the sea; and this seems to be true both in North America and in Asia, where the outflow in the plateau of Deccan has always excited the wonder of the naturalist.

That all the faults of the earth's crust depend on the sea and are produced by world-shaking earthquakes, is clearly indicated by the geographical distribution of these cracks in the crust. If any other cause, such as the secular cooling of the globe, were at work, we should find a relatively greater predominance of faults far inland, which is contrary to observation, especially in dry countries.

It is remarkable that geologists have referred so many phenomena to faulting, but have made little or no attempt to explain faulting itself. In the present theory referring the origin of faults to the expulsion of lava from under the sea we have for the first time a satisfactory and consistent view of these phenomena. Faults evidently arise mainly from the motion of lava in earthquakes, by which the overlying rocks of the crust are broken, and often displaced along the line of fracture.

When the crust is thus rent into blocks, some of them are reduced to small size, and eventually raised up, as in the vertical walls of granite now seen in Smyth's channel, southern Chile, the Straits of Magellan, Yosemite Valley, California, and the fiords of Norway. These precipitous walls of granite could be pushed up only by vertical forces, in earthquakes. It is noticeable that no such isolated masses are found towering up in the plains of Kansas, the desert of Sahara, and other inland regions far from the oceans. The origin of faults and fault movements must therefore be sought in the leakage of the oceans and in the resulting relief, which takes place in the sea bottoms and along the borders of the continents.

§ 26. *On the Uplift of the Great Plateaus of the World and on the Gradual Elevation of the Continents.*—For reasons already amply set forth in § 7, the process involved in the formation of the Andes is clear and beyond dispute. Now it happens that the Andean plateaus, such as those of Quito, Caxamarca, Cuzco and Titicaca, are generally included between the eastern and western ranges of the Andes, and were evidently uplifted by the same forces which formed the mountains themselves. Accordingly it is clear that a plateau such as that of Titicaca was therefore uplifted by the expulsion of lava from under the sea.

If now we pass from the Andes to the Himalayas, we shall find that in like manner those great mountains of Asia were uplifted principally by the Indian Ocean. The plateau of Thibet in the Himalayas of Asia corresponds exactly with that of Titicaca in the Andes of South America; and as the latter was formed with the Andes, so also the plateau of Tibet was formed with the Himalayas. This seems absolutely clear and incontrovertible. And a similar mode of development must be ascribed to the table lands to the east and west of Tibet, so that the principal plateaus of Asia, Tibet and Iran, are clearly the work of the sea.

The highest part of these plateaus is Tibet, with an average elevation of about 15,000 feet, and a width of about 500 miles at the highest part. At the middle it is somewhat wider, and to the west it narrows into Little Tibet, less than half the width of Tibet proper. It is evident that great Tibet was uplifted chiefly by movements from the direction of the bay of Bengal; this is shown by the lay of

the mountain chains south of Tibet, and by the great earthquake belt still persisting in the valleys of the Ganges and Brahmaputra.

In the case of North America the plateaus are broader and correspondingly lower than those of South America and Asia. But if the sea gave rise to the uplifts connected with the Andes and Himalayas, can anyone doubt that the plateaus of North America are due to the same cause? The *total volume* of the North American

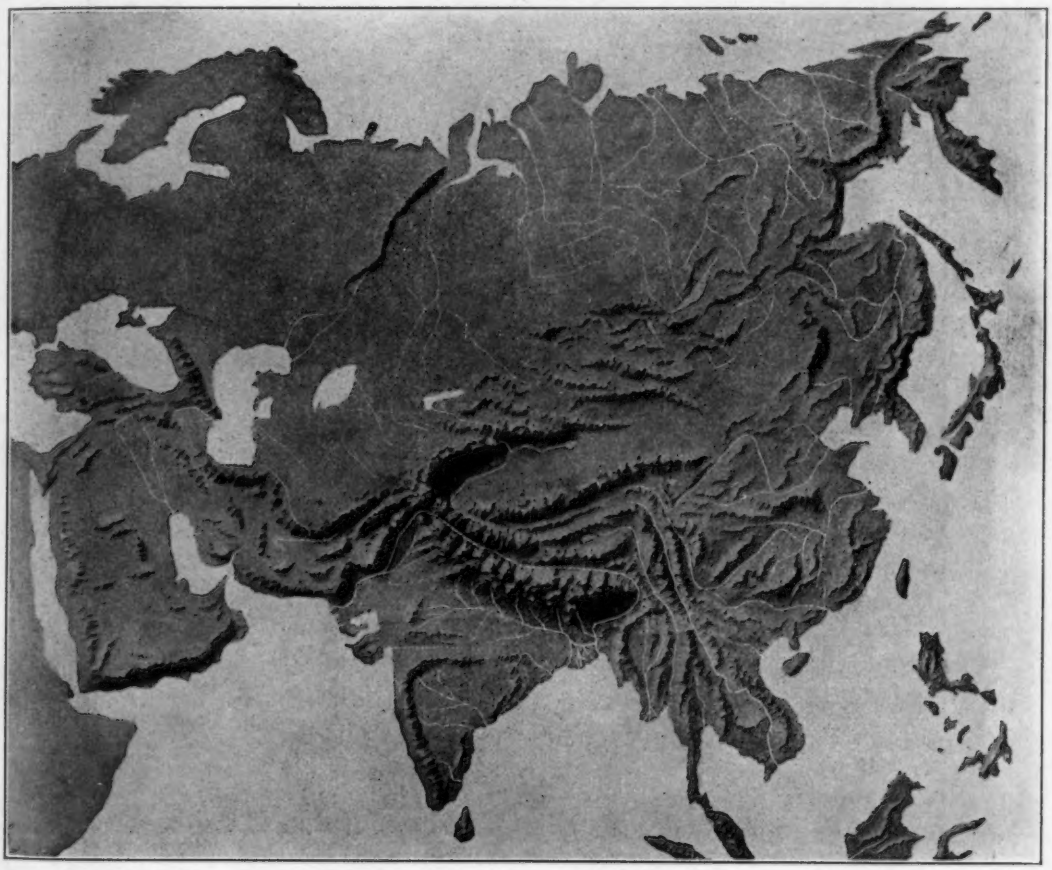

FIG. 5. Relief Map of Asia. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) The mountains along the east coast illustrate the successive stages in the recession of the Pacific Ocean. At some future time the border of the continent will extend to the string of islands running from Kamchatka to the Philippines, the shallow seas of Japan and China becoming inland valleys.

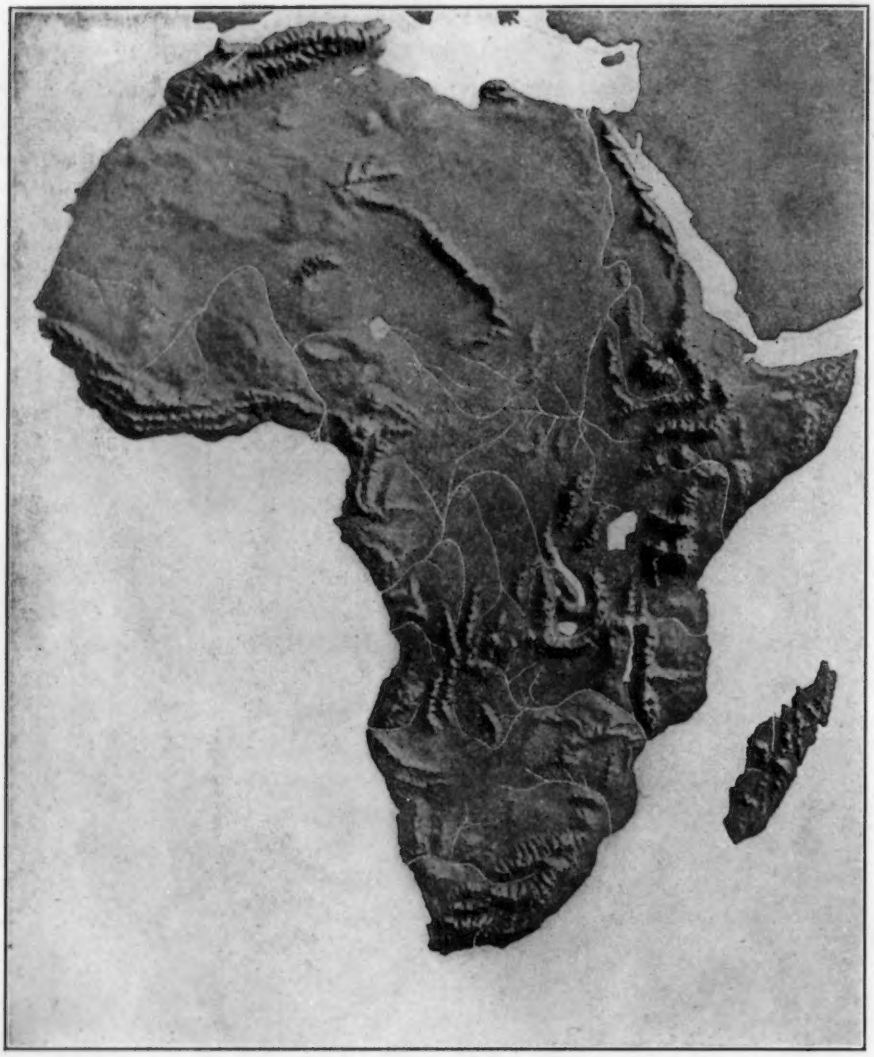

FIG. 6. Relief Map of Africa. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) The reader should notice how the highest mountains along the east coast face the Indian Ocean, which is a continuation of the Pacific.

plateau is comparable with that in Asia, and it is easy to see how the relief of the Pacific on our side may have taken the form of a table-land of greater width but smaller height. The numerous parallel mountain chains west of the Rocky Mountains show the nature of the mighty forces at work, and prove that this uplift was the work of the Pacific Ocean.

§ 27. *The forces which have raised the mountains and plateaus of the globe are identical with those which have raised the continents above the sea, and all these forces depend on the leakage of the oceans.*—The geological evidence of the slow operation of the forces which have uplifted the plateaus and mountains shows the immeasurable ages during which they have been at work. Sometimes large portions of a continent have risen for a time, and again slowly subsided, and thus have arisen the phenomena noted in the sedimentary rocks studied in geology. These gentle movements often are without violent earthquake shocks, because the yielding is very gradual, and the crust is slowly raised up and down without breaking. It is only where the expulsion of lava from under the sea is rapid and violent that breaking develops at such rate as to form mountain chains and plateaus. The uplift of a plateau also requires a large amount of material. Where the process is gentle and gradual a whole continent may be slowly uplifted, and this process evidently has raised the low broad plains above the water. *The cause of epeirogenic and of orogenic movements is everywhere one and the same.* The movements take different forms according to the suddenness with which the forces act; but both depend on the leakage of the oceans, and not at all on the secular cooling of the globe, the effect of which is insensible.[1]

[1] Since this was finished the writer has carefully recalculated the shrinkage of the earth's radius in 2,000 years, and finds that it can not exceed 1.5 inches. This takes no account of the increase of the interior heat of our globe due to radio-activity. If this latter effect were taken into account probably there would be no shrinkage whatever. Quite independently of these effects, however, there is an actual expansion of the globe due to the leakage of the oceans.

In the same way it is found, by the application of Fourier's theory of heat to the cooling at the surface, that the total shrinkage in the *length* of a continent such as North or South America, assumed to be equal to the terrestrial radius in length, is less than 1.5 inches. This again takes no

Such an inference seems justified by the study of the mountains and plateaus of the world, and also by the movement of the strand line which Professor Suess has so carefully traced in every country. Almost everywhere the level of the sea has been lowered in recent geological time.

During his travels in South America, Darwin recorded many observations to show that Patagonia and the whole end of the continent south of the La Plata had been recently elevated above the sea; and he mentions a channel in the Andes quite a distance north of the Straits of Magellan which gave evidence of the former passage of the sea through it. In view of these well-established facts, can any one doubt that the Straits of Magellan will eventually become dry and Tierra del Fuego be added to Patagonia? This whole region shows vast walls of rock towering vertically thousands of feet above the sea; evidently they were uplifted by earthquake forces from beneath, sometimes working quietly, and again spasmodically.

As surely as Calabria in Italy has been uplifted from the Mediterranean, by that sea, just so surely has the southern end of South America been raised up by the southern ocean. And if an end of a continent can be upraised, obviously whole continents can be uplifted. Accordingly in the leakage of the oceans and the relief taking place under the land which bounds them we have the true cause of continent-making.

Some original inequalities of surface may have existed after the detachment of the moon from the consolidating globe, but these have since been enormously increased by the effects resulting from the leakage of the oceans. As the earth gets older, the lithosphere becomes more diversified, and the face of the earth more and more wrinkled.

The situation of the great plateaus of the world facing the largest oceans gives a clear indication of the nature of the forces at work

account of radium, the effect of which would be to diminish this calculated shrinkage, or do away with it entirely. By such comparisons as these, placed along side of the large horizontal and vertical movements noticed in earthquakes near the sea, which sometimes amount to from 30 to 50 feet at a single disturbance, we see the utter untenability of the old theories heretofore current in works on geology and the related sciences. Note added July 28, 1908.

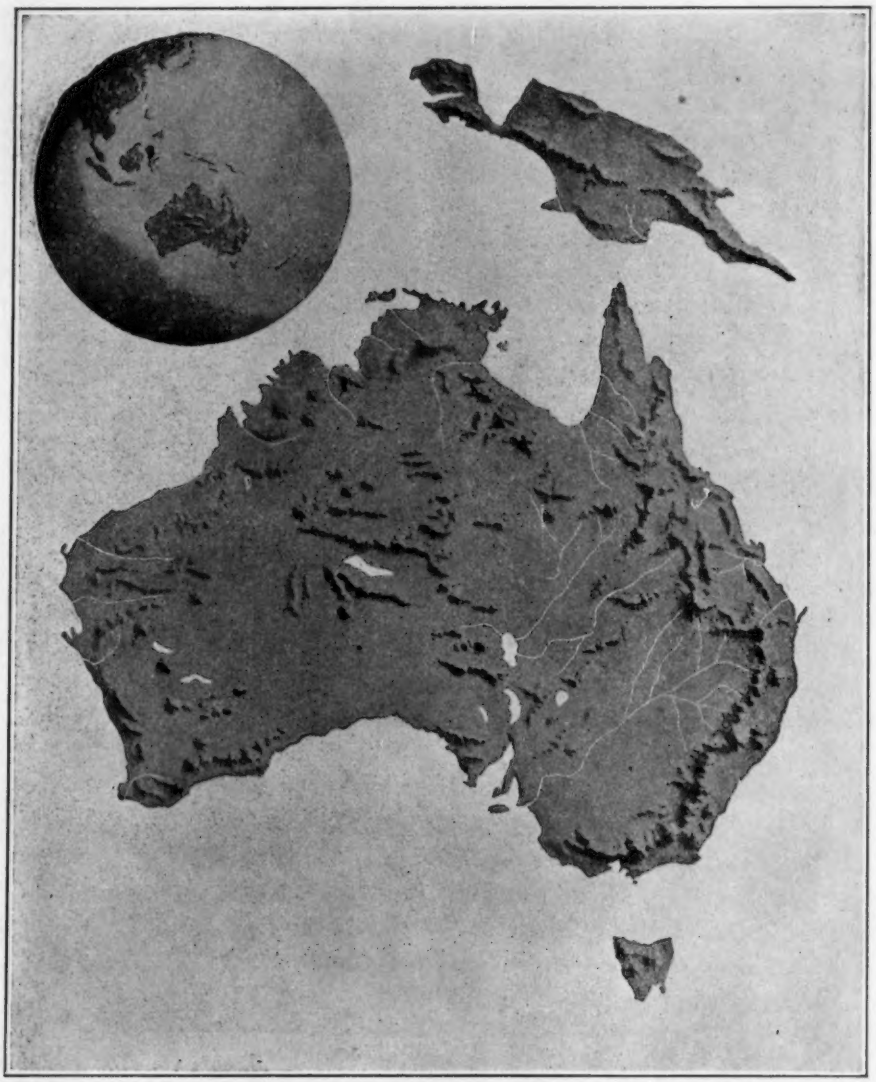

FIG. 7. Relief Map of Australia. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) The reader should notice how the largest mountains along the east coast face the Pacific Ocean.

FIG. 8. Relief Map of Europe. (From Frye's Complete Geography, by permission of Ginn and Co., Publishers.) The reader should notice how the principal mountain chains face the Mediterranean and the Atlantic. There is a trough in the sea bottom off the Scandinavian coast to which Professor Schiaparelli has called attention.

by which these mighty uplifts have been produced. The complex folding of the mountains to the east of Tibet shows that the Pacific aided the Indian Ocean in producing this great uplift, but we cannot yet determine the relative importance of the parts played by the two oceans.

§ 28. *On the Origin of the Alps and on the Extreme Crumpling and Folding which They Exhibit.*—The remarkable crumpling and folding noticed in the Alps has long been a matter of surprise and wonder to the naturalist. This phenomenon has always presented great difficulty to those who have attempted to explain the origin of the Alps. In the paper on the "Cause of Earthquakes" (§§ 14, 16, 18, 23) we have outlined the theory of how the Alps were formed by the sea, and criticised the old theories as totally inadequate to account for the observed crumpling. We propose here to develop the new theory a little further, and to show how it accounts for all the facts observed in a range such as the Swiss Alps, which are generally recognized as about the most complex system of mountains known upon the globe. If the new theory will explain the Swiss Alps, it will obviously explain any other mountain system in the world. The test of the theory as applied to the Alps may therefore be regarded as an *experimentum crucis.*

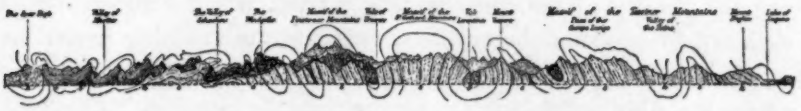

FIG. 9. Complex Folding. Section Across the Alps from the Neighborhood of Zürich toward Como; about 110 miles. (Heim and Prestwich.)

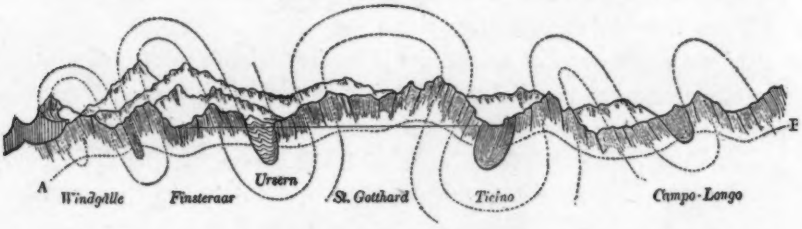

FIG. 10. Section through the Alps, Showing the Effects of Complex Folding. (From Heim's Gebirgsbildung.) The line of the St. Gotthard Tunnel and the plane of equal temperature, *AB*, beneath it, are compiled from F. Giordano, in *Bolletino del R. Comitato Geologico d'Italia*, Vol. XI., 1880, pp. 408–50.

The accompanying figures exhibit: (1) A general section of the Alps from Zürich to Lake Como (Heim and Prestwich), and (2) a section on a larger scale of a portion of the central Alps (from *Heim's Gebirgsbildung*) with fan-shaped folds and inversion of strata on the two sides. It can hardly be assumed that these illustrations are extremely accurate, but no doubt they are free from large errors in exhibiting the general character of the folding, which gives here and there fan-shaped structures with overturned dips at the sides.

Now the explanation of such structures is the most difficult problem heretofore presented to the geologist. They exhibit conspicuous lateral and vertical movements which cannot well be accounted for by the contraction theory. A shortening of about 74 miles (Heim) in the folding, which has amounted to 50 per cent. of the whole span of crust (Leconte), can not be accounted for, on the old theory, without assuming that the crust is loose from the globe, so that a vast amount of slack could be brought forward and concentrated in the folds at one point, in the Swiss Alps. This is clearly unthinkable. On the other hand, the cone of matter underlying the Alps with vertex at the center of the earth could not be sufficiently condensed to give the required slack in the overlying crust without increasing the density of the cone by 50 per cent., which could easily be detected by geodetic observations, owing to the resulting deviations of the plumb line. Accordingly we may feel sure that the matter under the Alps not only is not denser than the average, but actually lighter, by an appreciable amount. The crumpling of the Alps cannot therefore be due to condensation beneath these mountains.

How then did the folding arise?

If we cut a section across the Aleutian Islands perpendicular to the chain and the parallel trench lying to the south, we shall have a figure something like that shown in figure 1 of the following plate. Now in the paper on the "Cause of Earthquakes" (§ 16) we have shown how the undermining of the sea bottom sinks the trough down deeper and deeper, and as the expulsion of lava continues it eventually becomes easier to fold up the side of the trough towards the ocean (at *b*) and make another range of mountains parallel to the first. And there is nothing to prevent the process from being re-

peated several times. When several successive ranges of mountains are thus developed in the process of expulsion under the margin of the sea, it is easy to see that the central range may finally be driven upward and flared out at the top exactly as in the Alps. Thus all this movement occurs in the sea, and eventually the range becomes like that now seen in Switzerland, as depicted by Heim, of Zürich.

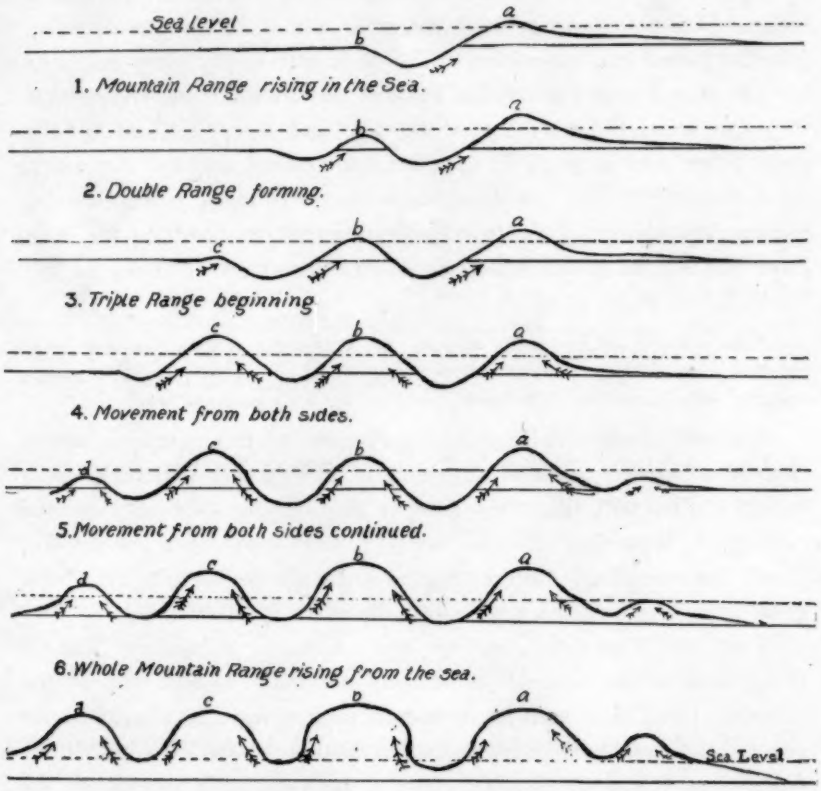
Sea Level
a
b
1. Mountain Range rising in the Sea.
2. Double Range forming.
c
3. Triple Range beginning
4. Movement from both sides.
d
5. Movement from both sides continued.
6. Whole Mountain Range rising from the sea.
Sea Level
7. Rising from the sea continued, giving fan-shaped structures and overturned dips.

FIG. 11. Illustration of Formation of Complex Range, such as the Swiss Alps. The bending of the crust has caused it to pull apart at the top and bottom of the folds, where it is largely covered by sedimentary deposits and filled by molten rock from beneath, so that the breaks do not show at the surface, unless erosion has laid bare parts of the underlying structure. In these figures the thickness of the crust is less than half the width of the folds; and for clearness the depth of the sea is exaggerated.

As the movement continues the central range rises upwards, while its flanks sink down on either side, and thus the fan-shaped structure develops, so as to give overturned dip and inversion of strata once deposited horizontally in the bed of the sea.

This is a perfectly simple and direct explanation of one of the most mysterious phenomena heretofore encountered by naturalists.

The new theory of mountain formation is proved to represent a real law of nature by phenomena now witnessed in the Aleutian Islands, Japan and elsewhere. The fact that it perfectly accounts for the perplexing phenomena seen in the Swiss Alps, shows that they too were formerly under the sea, and were uplifted by the same force now at work in the Aleutian Islands and the Antandes.

Accordingly it is not remarkable that Professor Suess should, without knowledge of the *true cause*, describe the uplift of the Alps from the sea in words which are almost prophetic ("Face of the Earth," Vol. II, p. 552):

> "As a result of *tangential thrusts*, the sediments of this Sea (Mediterranean) were folded together and driven upward as a great mountain range, and the Alps have therefore been described as a compressed sea."

Without overestimating the significance of this result, it seems clear that neither parallel ranges nor fan-shaped structures with inverted dips will hereafter present any further difficulty to the geologist. Now that the true laws of such phenomena are known, it will be exceedingly interesting to work out the details of all the great mountain systems with which the earth is adorned.

§ 29. *All Complex Folding now seen in Mountain Ranges Originated in the Sea.*—It is scarcely necessary to add that all the complex folds now seen in mountain ranges were produced in the sea by the repetition of trenches dug out by earthquakes. The folds were frequently broken apart at both top and bottom, by the earthquake movements, and thus the folded crust is not shortened by anything like so much as has been supposed. Moreover where the fan-shaped structures and overturned dips appear, the two sides were never joined together by an arch above, as represented in the above figures by Heim, but were quite separated before the range arose to any considerable height. Accordingly it follows that erosion has not worn off anything like so much of the top of the range as

the theory of a rounded arch would require. Thus we may not only explain the folds of the Alps, but also recognize that the folds both above and below were less extensive than was formerly supposed; and this greatly simplifies the labor of the geologist in restoring the former structure of mountain chains as they appeared before they were greatly eroded.

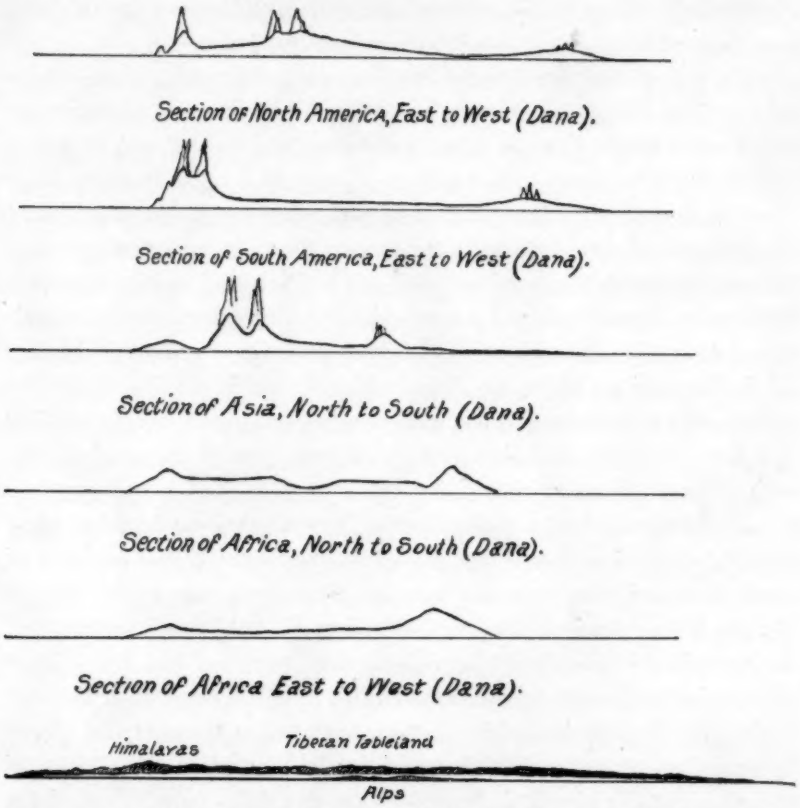
Section of North America, East to West (Dana).
Section of South America, East to West (Dana).
Section of Asia, North to South (Dana).
Section of Africa, North to South (Dana).
Section of Africa East to West (Dana).
Himalayas
Tibetan Tableland
Alps

FIG. 12. Sections of the Continents, and of the Alps and Himalaya on the same Scale. (Gen. Strachey.)

The process of undermining the sea bottom in the expulsion of lava arising from the leakage of the ocean, has given rise to all the important folds of the earth's crust. Thus arose all the complicated folds in the Alps, Andes, Alleghenies and other mountain ranges. And wherever we see these folds sea trenches once existed, and the

crust was pushed hither and thither by earthquakes, raising ridges and undermining the troughs, till the rocks were crumpled and folded as we find them to-day. *The simplicity of this cause, and the easy way in which we pass from the living troughs now being dug out in the sea to fossil troughs long since dead and now far inland give a genuine paleontological interest to the science of mountain formation.* What has long been mysterious and nearly inexplicable is now as clear as any theorem in geometry.

§ 30. *Application of the New Theory to the Allegheny Mountains.*—The Allegheny Mountains in Pennsylvania and Virginia are very remarkable for the great extent of the folds, and it seems worth while to dwell a moment on the mode by which these folds were produced. We have seen that they all arose in the sea, and by a repetition of the earthquake process of digging out trenches along the ancient shore line. As we shall see in Part V, § 41 of this paper, Professor James Hall so long ago as 1857 announced to the American Association in session in Montreal that the enormous thickness of the formations along the Appalachian Chain in the United States was due to the prolonged accumulation of sediments over a sinking sea bottom, at the margin of the continent, where the marine currents allowed the material to deposit.

Obviously if sea trenches were dug out by earthquakes they would become the basins for the accumulation of a vast amount of detritus. And when several trenches were successively dug out in the sea bottom by earthquakes depending on the Atlantic, would not the resulting folds give us the Allegheny, Tuscarora and Blue Ridge Mountains of Pennsylvania and Virginia? The famous Shenandoah Valley in Virginia is nothing but an ancient sea trough; and Pennsylvania has many such valleys originally formed in the depths of the sea. This is clearly indicated by the beautiful parallelism of the mountain ranges.

It is noticeable that the sea trench south of the Aleutian Islands is remarkably straight, and one may easily predict that the ranges hereafter to be formed in the North Pacific Ocean will be remarkably parallel like those now seen in Virginia and Pennsylvania. Under the circumstances can any one doubt that the sea was once very deep near where the Blue Ridge stands to-day?

Excluding from consideration the crystalline belt on the east, Claypole estimated the shortening of the Appalachians in Pennsylvania at 46 miles. In the same way McConnell estimated that of the Laramide range in British America at 25 miles, and Leeonte that of the Coast Range in California at from 9 to 12 miles. Corresponding estimates have been made for many other mountain ranges; but, for reasons already given in dealing with the origin of the Swiss Alps, § 28, these estimates are too large. The crust was broken apart at both top and bottom when the ranges were in the sea, and the folds heretofore assumed to be complete were never really so. Consequently no slack in the earth's crust is required to explain these folds; it was never loose from the globe and never moved horizontally, except when forced by earthquake movements proceeding from the underlying trenches in the sea bottom.

The undermining and folding of the crust has given the Appalachian Mountains in many places the aspect of a series of immense billows, running parallel, as if swept in by a vast disturbance of the sea. But not even seismic sea waves of the most imposing magnitude could approach the size of these gigantic folds, the origin of which heretofore has been so mysterious. The finding of a simple and natural explanation of *these great billows of the land* will be scarcely less interesting than the discovery of the cause of seismic sea waves. Both depend on earthquakes, though in very different ways. The land billows are cumulative products of an infinite series of seismic disturbances along the margin of the sea; the seismic waves are small in comparison, and result from a single disturbance of the sea bottom, made in process of shaping the vast billows of the land, which in all generations have appealed to the imagination of the painter, poet, and student of nature.

§ 31. *Analogy Between the Uplift of the Islands of Japan by the Movement from the Tuscarora Deep and of the Plateau of Tibet from the Indian Ocean.*—The uplift of the Islands of Japan now going on by the expulsions of lava from beneath the Tuscarora Deep is proved by the terrible earthquakes and seismic sea waves afflicting that region, as well as by the historical fact that the east coast of Japan is known to be rising from the sea. Perhaps in general the movement is slow and insensible, but occasionally earthquakes

have produced large disturbances of the level. The nature of earthquake movement in expelling lava from under the sea is too well known to leave any doubt as to what is going on in Japan. And the theory is confirmed by the fact that if Nipon and Yezo were dug off and thrown into the Tuscarora Deep they would about fill up that profound abyss and leave the sea of average depth.

Now there is a certain analogy between the uplift of these Japanese Islands, which are considerable areas, by the Pacific, and of the Plateau of Tibet by the Indian and Pacific oceans combined. Undoubtedly the valleys of the Indus, Ganges and Brahmaputra are the relics of ancient sea troughs which largely produced the Himalayas and the great plateau of Tibet. How much these troughs have been modified in later geological times we cannot estimate; but even now enough remains to tell the true story of Himalayan development. This is also indicated by the preservation of the earthquake belt south of the Himalayas. The meaning of these valleys and earthquake belts admits of no possible doubt. Just as the whole island of Nipon is being raised by movements from the Tuscarora Deep, so the whole of the Plateau of Tibet was once raised by an Indian Deep, of which these valleys are the remains.

In the same way the Valley of the Po is the remains of the sea valley which was most influential in uplifting the Swiss Alps. But in the case of the Alps, Geikie has shown that there was also a sea on the north, which has now quite disappeared, though traces of its former existence still remain.

§ 32. *The Origin of Volcanoes and the Conditions of their Maximum Development.*—It appears from the line of proof developed in this theory that volcanoes may break forth in any region near the sea where there are severe earthquake disturbances, by which the crust of the globe is sufficiently cracked to afford a vent for the steam imprisoned beneath. Now such vents are greatly facilitated in a chain such as the Aleutian Islands, in which the crumpling is extreme, and the expulsion of lava from beneath the sea rapid and violent. The crumpling breaks the crust along many lines, and as the earthquakes due to the expulsion of lava are both frequent and terrible, the chance of steam breaking through to the surface

is much greater than in regions less wrinkled and less afflicted by earthquakes. The crust in the Andes was once folded by the sea in the same way as that in the Aleutian islands, and from this circumstance arises the violence of the volcanic outbreaks noticed all along the west coast of South America. From the great similarity of the volcanic phenomena in the Andes and in the Aleutian Islands, and its enormous prominence in both ranges, it seems obvious that we have here the conditions for its maximum development.

Charles Darwin believed that volcanoes usually break out in regions of elevation. No doubt this is true, for mountain ranges are the most conspicuous of rising areas. And according to this theory the tendency to rupture the crust is a maximum, when the ranges are being both folded and raised from the sea. Thus while some volcanoes may break out in less fractured regions of the earth's crust, the greatest volcanic activity develops where mountains are being formed in the sea, as in the Aleutian Islands. This view also enables us to understand why many volcanoes in the Andes are now extinct, though they were formerly active for immense periods of time, as we know from the thick deposits of volcanic debris and the immense height of the cones built up of lava, ashes and cinders.

IV. Comparison of the New Physical Theory of Mountain Formation Depending on the Leakage of the Oceans with the Theory of Secular Cooling and Contraction Heretofore Held by Men of Science.

§ 33. *General Remarks on the Method of Comparison Adopted.*— The new physical theory of mountain formation depending on the leakage of the oceans outlined in the three memoirs recently published by the American Philosophical Society and somewhat more fully developed in the present paper might seem incomplete if we failed to compare the new theory with the theory of secular cooling and contraction of the globe heretofore held by men of science generally. On several grounds an examination of the older theory can hardly fail to be instructive. And if this comparison of the older theory with that now adopted shall be the means of har-

monizing in any considerable degree the divergent views heretofore prevailing, and of showing *that there is no important geological phenomenon which the new theory does not explain in a more simple and direct manner than the old theory,* such a comparison will no doubt seem quite justifiable. For it is highly desirable to establish the adequacy of the new theory to explain the *geological* as well as the *physical* phenomena noticed at the surface of the earth.

In making this comparison it is necessary to bear in mind that the geological data on many points are still very incomplete, and therefore we should expect agreement with the body of phenomena rather than with the details, about which much uncertainty still exists. Owing to the incompleteness of our knowledge of the mode of origin of the great mountain chains of the globe, the best plan of procedure seems to be: First, to give an exposition of the views of previous writers in regard to the individual great mountain systems; second, to add a résumé of the views of certain great geologists on mountain formation in general. Obviously such condensation of the views of others should wherever possible be given in their own words.

As this subject is extensive and widely scattered in a variety of publications, we must content ourselves with selecting those citations which seem of most interest, without in any way claiming to exhaust the subject. Indeed it may well be that some discussions of value will be entirely overlooked, but, as the theories have been but very little changed for many years, it is hoped that the following citations will be found adequate to give an intelligent grasp of the views heretofore accepted by the leading authorities. If there be those who doubt the propriety of including lengthy quotations from well-known authors, I must plead in extension of the course here adopted, that this memoir is intended for others besides geologists, and that all who are interested in the physics of the earth, whether they be mathematicians, astronomers, physicists, seismologists, geologists, or even chemists and biologists, are entitled to have a clear summary of the principal theories heretofore accepted in regard to the development of our globe. In dealing with a subject of such universal interest to all men of science, any reasonable condensation of the previous theories may be considered admissible, and one

may have no hesitation in invoking the aid of many authors. If the establishment of a great law of nature may be thus facilitated, surely no one will doubt that the space utilized was devoted to a most useful purpose. The extreme specialization characteristic of the science of our day makes such summaries both useful and necessary for the intelligent study of great problems; and if more effort were made in this direction it might contribute materially to the progress of scientific research.

(A) Accounts of Particular Mountain Systems, and Their Supposed Mode of Development.

§ 34. *The Andes.*—We shall begin with the Andes of South America, because this is one of the largest, simplest and most typical of mountain systems; and if a theory will not explain the Cordilleras, we may despair of its explaining the more complicated mountains of the globe. The reader should carefully bear in mind not only what the author in question says from his own point of view, but also how the facts he mentions accord with the new theory developed in this paper.

In the *Encyclopedia Britannica,* ninth edition, under the article "Andes," we find the following lucid exposition of Andean development. It is not signed, but is supposed to have passed under the review of Sir Archibald Geikie.

"The formation of the Andes is due to several causes operating at distinct intervals of time. They consist mainly of stratified material which has been more or less altered. This material was deposited at the bottom of a sea, so that at some former time the highest portions were submerged, probably in consequence to a certain extent, of subsidence of the sea bottom. Since the latest deposits there has been upheaval and denudation. The range, then, has resulted from the accumulation of sediment on a subsiding area; from the subsequent upheaval of such deposits, which have been increased in height by the ejection of volcanic products; and from the operation of denuding agents.

"As far as our present knowledge goes, it appears to be probable that the Andes mark an area on which sedimentary deposits have been accumulated to a greater thickness than on any other portion of South America. It is further demonstrable that these deposits belong to several geological periods, the elevation having occurred at different periods, while their axes extend in different directions. Hence it is a complex range of mountains formed by the combination of several distinct systems of ridges. The width of the

range varies from about 60 to 300 or more miles, but, as compared with other mountains, the Andes are for the most part narrow relatively to their height. Where their special features are most characteristically developed, they consist of a massive embankment-like foundation, rising with a rapid slope from the low country on either side, and having its margins surmounted by lofty ridges of ragged or dome-like summits. These Cordilleras, as they are usually termed, flank longitudinal valleys, or plain-like depressions which form the highest levels of the central portion of the gigantic embankment, and which vary in width from twenty to sixty miles. At intervals the longitudinal depression is broken up, either by ridges connecting the Cordilleras, or by lofty plateau-like uplands. In several cases these transverse ridges and belts of high ground form the main watershed of the country. They are rarely cut across by the river systems, whereas both the marginal Cordilleras are intersected at numerous points, and more especially by the rivers draining the eastern slope of the country. In no case do these eastern rivers originate to the west of the western Cordilleras. A few of the central valleys, or plain-like depressions, have no connection either with the western or eastern river system. Roughly speaking the height of the central plains or valleys is from 6000 to 11,000 feet above the sea; of the passes and knots, from 10,000 to 15,000 feet; and of the highest peaks, from 18,000 to 23,290 feet—the last being the altitude of Aconcagua in Chili, which is generally considered to be the highest peak in America. Judging from these estimates, we may regard the bulk of the Andes as somewhere about that of a mass 4400 miles long, 100 miles wide, and 13,000 feet high, which is equivalent to 5,349,801,600,000,000 cubic feet. On this basis we find that the Mississippi would carry down an equivalent mass of matter in 785,000 years. The rate of denudation in certain river basins varies from one foot in 700 years to one foot in 12,000 years. Assuming that similar rates would apply to the Andes, they would be denuded away in from 9 to 156 million years. In all probability, much less than 9 million would suffice. On the other hand the Andes would be swept away in 135,000 years, supposing the denuding powers of the globe were concentrated on them alone. From the above data, and assuming the average specific gravity of the matter forming the Andes to be 2.5, the weight of the portion above the sea may be estimated at 368,951,834,482,750 tons, giving an average of about 1,000 tons on each square foot at the level of the sea. Under Aconcagua the pressure would be about 1,780 tons per foot at the same level, provided, of course, it were not, as it no doubt is, more or less modified by lateral pressure. These figures afford some, though at best a vague, conception of the mighty grandeur of this range of mountains, and of the scope there is for the exertion of enormous pressure. How vast then, must be those forces which have counteracted such pressures, and upheaved the ocean-spread sediments of the continents, until the Andes, that

'giant of the Western Star,
Looks from his throne of clouds
O'er half the world!'

But, however vast the Andes may seem to us, it should be remembered that they form but an insignificant portion of the globe itself. Aconcagua is about 1/2,000 of the earth's diameter, which is relatively not more than a pimple 1/30 of an inch high on the skin of a tall man." (Ency. Brit., Vol. II, pp. 15–16.)

The account here given of how the Andes were formed seems exceedingly instructive. In the sea troughs formerly existing between the ocean and the eastern range, which was the first thrown up, we have a complete explanation of the extraordinary depth of sedimentation; for in such trenches adjacent to a new range the rate of sedimentation would be a maximum. The subsequent uplifting of the western side of the sea troughs, with the vast lateral folding and compression necessarily accompanying this movement, accounts for the plateaus, valleys and general structure of the Andes, as well as for the violent volcanic outbreaks, which are said to greatly predominate in the range nearest the sea, from which the expulsion of lava giving rise to this mighty Cordillera proceeded. The vastness and height of the Andes and the terrific forces operating to erect this gigantic wall along the shore of the continent is a true measure of the secular leakage of the Pacific Ocean, and of the automatic relief it finds by folding the earth's crust along the border, in the countless successive expulsions of lava from beneath the bed of the sea. It is needless to point out how perfectly the new theory explains the persistence of the earthquake belt along the western shore of South America, and of the seismic sea waves by which that region is so often afflicted. It is obvious that the forces which uplifted the mountain also carried up the plateaus enclosed between the various ranges.

§ 35. *The Himalayas.*—The following luminous account of the Himalayas by the late Lieutenant General Sir Richard Strachey, *Encyclopedia Britannica,* article "Himalayas," is of extreme interest. General Strachey resided in India for many years, and made a life long study of the Geology and Geography of Central Asia. He was the principal authority of his time on this little explored continent and died February 12, 1908, at the age of 91 years.

"Scientific investigation has clearly shown that, so far as the main characteristics of the mountains are concerned, the natural boundaries of the Himalayan system must be carried much farther than had at first been

recognized. Considerable obscurity still involves the eastern portion of these mountains, and there is great want of precise knowledge as to their connection with the ranges of western China, from which are thrown off the great rivers of China, Siam, and Burmah. On the west, however, it has been completely established that a continuous chain extends beyond the Indus along the north of the Oxus, and ends in that quarter about 68° E. long. In like manner it is found that no separation can be established, except a purely arbitrary one, between the Himalaya as commonly defined and the greatly elevated and rugged table-land of Tibet; nor between this last and the mountain ranges which form its northern border along the low-lying desert regions of central Asia.

"It thus appears that the Himalaya, with its prolongation west of the Indus, constitutes in reality the broad mountainous slope which descends from the southern border of the great Tibetan table-land to the lower levels of Hindustan and the plains of the Caspian; and that a somewhat similar mountain face, descending from the northern edge of the tableland, leads to another great plain on the north, extending far to the eastward, to the northern borders of China. Towards its northwest extremity this great system is connected with other mountains—on the south, with those of Afghanistan, of which the Hindu-Kush is the crest, occupying a breadth of about 250 miles between Peshawur and Kunduz; and on the north, with the mountains that flank the Jaxartes or Sir on the north, and the Thian-shan or Celestial Mountains. The eastern margin of Tibet descends to western China, and the south-eastern termination of the Himalaya is fused into the ranges which run north and south between the 95th and 100th meridians, and separate the rivers of Burmah, Siam, and western China.

"Nor can any of the numerous mountain ranges which constitute this great elevated region be properly regarded as having special, definite, or separate existence apart from the general mass of which they are the component parts; and Tibet cannot be rightly described, as it has been, as lying in the interval between the two so-called chains of the Himalaya and the Kouenlun or Kara Koram. It is in truth the summit of a great protuberance above the general level of the earth's surface, of which these alleged chains are nothing more than the south and north borders, while the other ranges which traverse it are but corrugations of the mass more or less strongly marked and locally developed.

"The average level of the Tibetan tableland may be taken at about 15,000 feet above the sea. The loftiest points known on the earth's surface are to be found along its southern or Himalayan boundary; one of them falls very little short of 30,000 feet in elevation, and peaks of 20,000 feet bound the entire chain. The plains of India which skirt the Himalayan face of the tableland, for a length of rather more than 1,500 miles, along the northern border of British India, nowhere rise so much as 1,000 feet above the sea, the average being much less. The low lands on the north, about Kashgar and Yarkend, have an elevation of from 3,000 to 4,000 feet, and no part of the Central Asiatic desert seems to fall below 2,000 feet, the lake of Lob-nor being somewhat above the level. The greatest dimen-

sion of the Tibetan mountain area from east to west may be about 2,000 miles, while its average breadth somewhat exceeds 500 miles; about 100 miles on either side constitute the sloping faces, the central tableland having a width of about 200 miles on the west and probably 500 miles at its eastern border."

General Strachey thus shows that the Himalayan mountains and Tibetan Plateaus are directly and intimately connected as merely different parts of one great continuous movement of the earth's crust.

After describing many features of the Himalayas, General Strachey continues:

"The general conclusion that may be drawn from the facts of structure thus briefly indicated is that the elevation of the Himalaya to its present great height is of comparatively recent occurrence. An area of land must have existed where the main line of snowy peaks now stands, which has not been submerged since the Palæozoic period, and which then had its northern boundary somewhere along what has been termed the Indian watershed. Evidence of a similar ancient sea on the south also exists, but in less definite shape; and whether it was united with the northern sea or not is still a matter of conjecture, though the distinctive character of the fossils rather indicates that there was no direct union. The possible connection of this ancient Himalayan land area with the pre-Tertiary land of the peninsula of India is also only a matter for speculation.

"There is further reason to infer that the existence of the great line of peaks is rather due to some previous line of elevation on the ancient land, which has continued to retain its relative superiority while the whole areas have been raised, rather than to any special line of energy of upheaval of recent date; and that the fundamental features of its former configuration of surface in mountain and valley have been preserved throughout. There is evidence for the conclusion that the chief rivers of the pre-Tertiary land issued from the mountains where the present main streams are found, and this embryo Himalaya may have been of such moderate height as to have permitted the passage across it of the Siwalik mammals, the remains of which appear both on the border of the Indian plain and in Tibet. It is after the middle Tertiary epoch that the principal elevation of these mountains must have taken place, and about the same time also took place the movements which raised the tablelands of Afghanistan and Persia, and gave southern Asia its existing outlines.

"The best answer that can be given to an inquiry as to how changes of level could have arisen, such as those which are observed in the Himalaya, is that they should be regarded as due rather to secondary actions consequent on the general contraction of the cooling terrestrial sphere than to direct elevating forces, for which no known origin can be assigned. The contraction of the cooling but now solid crust of the earth must have set up great horizontal strains, partly of tension and partly of compression

which would necessarily have been followed by rupture or crushing along lines of least resistance, and the movements on such lines are marked by the great mountain ranges that traverse the surface. A dislocation of the solid crust of the earth once having taken place, it would probably continue to be a line of least resistance ever after, and a succession of movements during past geological periods may thus be reasonably expected along such lines. Somewhat in proportion as the disturbing forces are intense, and the thickness of the crust on which they act is great, will be the tendency of the lines of rupture to be continuous for a considerable distance; and as the disturbed area is extended in its dimensions, the probability will increase of a repetition of a series of similar dislocations on lines approximately parallel to, or at right angles to, one another and to the line on which the greatest compression and consequent tension take place. In a disturbed area, one transverse dimension of which is sensibly greater than the rest, the longitudinal ruptures will predominate in the interior and the transverse towards the borders. Almost all mountains give indications of having been shaped by forces thus related, and to the action of such forces may the main characteristics of the structure of the Himalaya, and the arrangement of its ridges and valleys be attributed. Whatever may be the power of rivers in general as instruments of erosion, and whatever effect the Himalayan rivers have had in removing the fragments of the rocks over and among which they took their courses, it is hardly possible to doubt that their main directions were determined by the anterior lines of dislocation which opened up hollows down which they could flow, and which must invariably have been accompanied by a destructive and crushing action on the rocks along them, which has enabled the waters the more readily to sweep away the obstacles in their path. The parallelism of many of the great Tibetan and Himalayan rivers for hundreds of miles together, and such mountains, seems wholly inexplicable in any other manner." (Ency. Brit., p. 828.)

This account is quite clear and satisfactory, except that part of it which deals with the cooling and contraction of the globe. Here General Strachey has made the best of a very inadequate hypothesis.

Just as the Andes were formed by expulsions of lava from under the Pacific, so also here the Himalayas were formed by a corresponding movement due mainly to the Indian Ocean, which has also raised high mountains along the eastern border of Africa. We cannot yet give all the details of the Himalayan development, but in general it is evident that it was similar to that of the Andes. The uplift of the great plateau of Tibet corresponds to that of Titicaca. And the parallel ranges of the Himalayas originated by the usual process of the folding up of successive sea trenches. On the

outside of these mountains there still remain trough-like depressions where the Indus, Ganges and Brahmaputra now flow. The undermining produced in raising the Himalayan embankment still shows in the valleys to the south, though the sea has receded; and the great earthquake belt south of the Himalayas still discloses to us the nature of the forces which produced this mighty uplift.

The following critical passages by General Strachey are also of decided interest:

"The great peaks are, with few exceptions, composed of schistose rock, though granite veins may be seen in the mountain faces to very great elevations; one of these exceptions is the great peak of Kamet in Kumaon, which rises to about 25,000 feet in what appears to be a mass of grey granite.

"Passing to the north of the line of great peaks the metamorphosed schists are suddenly replaced by slates and limestones, which are in many places highly fossiliferous, exhibiting what appears to constitute in the aggregate a fairly continuous series from the Lower Silurian to the Cretaceous formations, though the complete sequence has not been observed in any one locality. The western region of the Himalaya alone has been sufficiently explored to admit of any positive statements, but the indications gathered from such imperfect accounts and other data as exist relative to the eastern parts of the mountains leave little doubt that the change observed in the west on approaching and entering Tibet holds good on the east also, and that the general physical features of the whole tract are much alike, though doubtless with many differences in detail.

"The fossiliferous strata of western Tibet are continued, though perhaps with some breaks, to the Tertiary period. In certain localities nummulitic rocks, probably Eocene, have been observed, and from the great alluvial deposit which forms the plain of Gugé, already noticed, the remains of mammals, apparently of Siwalik age, have also been obtained. Among these were bones of the elephant and rhinoceros, the existence of which, in the present condition of these regions, would be wholly impossible; so that there is no room to doubt that these deposits have been raised from a comparatively low level to their existing great elevation of upwards of 15,000 feet, since they were laid out. As in the case of the plain of India, we here, too, have no complete proof of the origin of these great nearly horizontal deposits, but it seems clear, from the materials of which they are formed, that they must have been laid out by the water, either by the sea or some great inland lake. They are largely composed of boulder deposits, and large boulders are strewed over the surface imbedded in the ground in a manner that seems only explicable as the result of the action of a considerable body of water.

"Several lines of granitic and eruptive rock occur in western Tibet, of which all that need here be said is that they appear all to be older than the Tertiary alluvium, but some of them are possibly contemporaneous with the nummulitic and older formations." (Ency. Brit. p. 828.)

In an earlier passage, after comparing some of the smaller Himalayan ranges to the Swiss Alps, General Strachey adds:

"To obliterate these two ranges from the Himalaya would make no very sensible inroad on it, though they surpass in bulk the whole of the Swiss Alps; and it is no exaggeration to say that, along the entire range of the Himalaya, valleys are to be found among the higher mountains into which the whole Alps might be cast without producing any result that would be discernible at a distance of ten or fifteen miles. And it is important to bear in mind these relations of magnitude, for the terms at our disposal in the description of the mountains are so limited that it is necessary to employ the words chain, range, ridge, spur, etc., rather with reference to relative than to absolute importance, so that the scale of our nomenclature changes with the extent and altitude of the mountains of which we speak." (Ency. Brit., p. 827.)

§ 36. *The Alps.*—In the *Enclycopedia Britannica,* article "Alps," by John Ball, we find the following brief outline of the salient features:

"Accurate knowledge of the Alps is so recent that few attempts have been made to establish a general division of the entire region, and it cannot be said that any one arrangement has obtained such general recognition as not to be open to future modification; but there is a pretty general agreement as to the main features of that here proposed, to which a few general remarks must be premised.

"Whatever may have been the original cause of the disturbances of the earth's crust to which great mountain chains owe their existence, it is generally, though not universally, true that the higher masses (formed of crystalline rock and geologically more ancient) are found towards the central part, and that these are flanked by lower ranges, composed of more recent rocks, which surround the central groups very much as an outer line of entrenchment may be seen to surround a fort. In most cases it is not possible to descend continuously in a nearly direct line from the crest of a great mountain chain to the plains on either side, for there are usually intermediate valleys, running more or less parallel to the central range, which separate this from outer secondary ranges. These in turn, are often accompanied by external ranges, intermediate between them and the plains, and related to them as they are to the central ranges. The type of arrangement here described is more or less traceable throughout the greater part of the Alps, but is most distinctly exhibited in the eastern portion lying between the Adige and the frontier of Hungary. We have a central range, composed mainly of crystaline rock; a northern range, formed of secondary rocks, separated from the first by the great valleys of the Inn, the Salza, and the Enns; a southern range, somewhat similar to the last in geological structure, divided from the central one by the Rienx, or east branch of the Adige, and the Drave. Flanking the whole, as an external entrenchment on the north side, are the outer ranges of the Bavarian Alps, of the Salzkam-

mergut, and of Upper Austria, to which corresponds on the south side the Monti Lessini, near Verona, the mountains of Recoaro, those of the Sette Comuni, and the considerable masses crowned by the summits of the Grappa, the Col. Vicentino, the Monte Cavallo, the Monte Matajur, and Monte Nanos. Where, as in the case above mentioned, the secondary ranges of the Alps rise to a greater altitude, and are completely separated from the neighbouring portions of the central chain, it is impossible not to distinguish them as distinct groups; but the outermost ranges, which rarely rise above the forest zone, are in all cases regarded as appendages of the adjoining groups. These outer ranges are called in German Voralpen, and in Italian Prealpi." (Ency. Brit., p. 623.)

Again on page 620, this author remarks:

"In every mountain system geographers are disposed to regard the watershed, or boundary dividing the waters flowing towards the opposite sides of the range, as marking the main chain; and this usage is often justified by the fact that the highest peaks lie on, or very near, the boundary so defined. In applying this term in the case of the Alps, there are, however, difficulties arising from their great extent and the number of their branches and ramifications. Many of the loftiest groups lie altogether on one side of that which we call the main chain, and at the eastern extremity, where all drainage is ultimately borne to the Black Sea, we must be partly guided by geological considerations in deciding which of several ranges deserves to be considered pre-eminent." (Vol. I., p. 620.)

Sir Archibald Geikie's discussion of the origin of the Alps, in the article "Geology," *Encyclopedia Britannica* (pp. 373-374), bears on the problem now before us:

"The Alps, on the contrary, present an instructive example of the kind of scenery that arises where a mass of high ground has resulted from the intense corrugation and upheaval of a complicated series of stratified and crystalline rocks, subsequently for a vast period carved by rain, frost, springs and glaciers. We see how, on the outer flanks of those mountains among the ridges of the Jura, the strata begin to undulate in long wave-like ridges, and how, as we enter the main chain, the undulations assume a more gigantic tumultuous character, until, along the central heights, the mountains lift themselves towards the sky like the storm-swept crests of vast earth billows. The whole aspect of the ground suggests intense commotion. Where the strata appear along the cliffs or slopes they may often be seen twisted and crumpled on the most gigantic scale. Out of this complicated mass of material the sub-aerial forces have been ceaselessly at work since its first elevation. They have cut valleys, sometimes along the original depressions, sometimes down the slopes. They have eroded lake-basins, dug out corries or cirques, notched and furrowed the ridges, splintered the crests, and have left no part of the original surface unmodified. But they have not effaced all traces of the convulsions by which the Alps were upheaved."

In his account of the Miocene ("Text-book of Geology," p. 1261, edition of 1903), Geikie says:

"The Gulf of Gascony then swept inland over the wide plains of the Garonne, perhaps even connecting the Atlantic with the Mediterranean by a strait running along the northern flank of the Pyrenees. The sea washed the northern base of the now uplifted Alps, sending, as in Oligocene time, a long arm into the valley of the Rhine as far as the site of Mainz, which then properly stood at the upper end, the valley draining southward instead of northward. The gradual conversion of salt into brackish and fresh water at the head of this inlet took place in Miocene time. From the Miocene firth to the Rhine, a sea-strait ran eastwards, between the base of the Alps and the line of the Danube, filling up the broad basin of Vienna, sending thence an arm northwards through Moravia, and spreading far and wide among the islands of southeastern Europe, over the regions where now the Black Sea and Caspian basins remain as the last relics of this Tertiary extension of the ocean across southern Europe. The Mediterranean also still presented a far larger area than it now possesses, for it covered much of the present lowlands and foot-hills along its northern border, and some of its important islands had not yet appeared or had not acquired their present dimensions."

On pages 1371-2 of Geikie's "Geology," we find the following interesting passages:

"*Alpine Type of Mountain Structure.*—It is along a great mountain chain like the Alps that the most colossal crumplings of the terrestrial crust are to be seen. In approaching such a chain, one or more minor ridges may be observed running on the whole parallel with it, as the heights of the Jura flank the north side of the Alps, and the sub-Himalayan hills follow the southern base of the Himalayas. On the outer side of these ridges, the strata may be flat or gently inclined. At first they undulate in broad gentle folds; but traced towards the mountains these folds become sharper and closer, their shorter sides fronting the plains, their longer slopes dipping in the opposite direction. This inward dip is often traceable along the flanks of the main chain of mountains, younger rocks seeming to underlie others of much older date. Along the north front of the Alps, for instance, the red molasse is overlain by Eocene and older formations. The inversions and disruptions increase in magnitude till they reach such colossal dimensions as those of the Glärnisch, where pre-Cambrian schists, and Triassic, Jurassic, and Cretaceous rocks have been driven for miles over the Eocene and Oligocene flysch (pp. 677, 693). In such vast crumplings and thrusts it may happen that portions of older strata are caught in the folds of later formations, and some care may be required to discriminate the enclosure from the rocks of which it appears to form an integral and original part. Some of the recorded examples of fossils of an older zone occurring by themselves in a much younger group of plicated rocks may be thus accounted for.

"The inward dip and consequent inversion traceable towards the center of a mountain chain lead up to the fan-shaped structure (p. 678) where the oldest rocks of a series occupy the center and overlie younger masses, which plunge steeply under them. Classical examples of this structure occur in the Alps (Mont Blanc, Fig. 258, St. Gothard), where crystalline rocks such as granite, gneiss, and schists, the oldest masses of the chain, have been ridged up into the central and highest peaks. Along these tracts, denudation has been of course enormous, for the appearance of the granitic rocks at the surface has been brought out, not necessarily by actual extrusion into the air, but more probably by prolonged erosion, which in these higher regions, where many forms of sub-aerial waste reach their most vigorous phase, has removed the vast overreaching cover of younger rocks under which the crystalline nucleus doubtless lay buried."

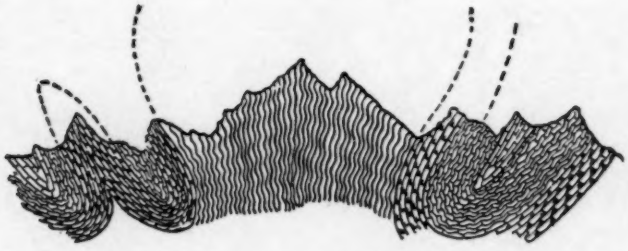

FIG. 13. Fan-shaped Structure, Central Alps.

Again on page 1372, we read:

"A mountain chain may be the result of one movement, but probably in most cases is due to a long succession of such movements. Formed on a line of weakness in the crust, it has again and again given relief from the strain of compression by undergoing fresh crumpling and upheaval. Successive stages of uplift are usually not difficult to trace. The chief guide is supplied by unconformability. . . .

"In most great mountain chains, however, the rocks have been so intensely crumpled, dislocated, and inverted, that much labor may be required before their true relations can be determined.

"The Alps offer an instructive example of a great mountain system formed by repeated movements during a long succession of geological periods. The central portions of the chain consist of gneiss, schists, granite, and other crystalline rocks, partly referable to the pre-Cambrian series, but some of which (Schistes lustrés, Bündnerschiefer) include metamorphosed Palæozoic, Secondary, and in some places, perhaps, even older Tertiary deposits (pp. 802, 1099). It would appear that the first outlines of the Alps were traced out even in pre-Cambrian times, and that after submergence, and the deposit of Palæozoic formations along their flanks, if not over most of their site, they were reelevated into land. From the relations of the

Mesozoic rocks to each other, we may infer that several renewed uplifts, after successive denudations, took place before the beginning of Tertiary times, but without any general and extensive plication. A large part of the range was certainly submerged during the Eocene period under the waters of the wide sea which spread across the center of the Old World, and in which the nummulitic limestone and flysch were deposited. But after that period the grand upheaval took place to which the present magnitude of the mountains is chiefly due. The older Tertiary rocks, previously horizontal under the sea, were raised up into mountain-ridges more than 11,000 feet above the sea-level, and together with the older formations of the chain, underwent colossal plication and displacement. Enormous slices of the oldest rocks were torn away from the foundations of the chain and driven horizontally for miles until they came to rest upon some of the newest formations. The thick Mesozoic groups were folded over each other like piles of carpets, and involved in the lateral thrusts so as now to be seen resting upon the Tertiary flysch. So intense was the compression and shearing to which the rocks were subjected that lenticles of the Carboniferous series have been folded in among Jurassic strata, and the whole have been so welded together that they can hardly be distinguished where they meet, and what were originally clays and sands have been converted into hard crystalline rocks. It is strange to reflect that the enduring materials out of which so many mountains, cliffs, and pinnacles of the Alps have been formed are of no higher geological antiquity than the London Clay and other soft Eocene deposits of the south of England and the north of France and Belgium. At a later stage of Tertiary time, renewed disturbance led to the destruction of the lakes in which the molasse had accumulated, and their thick sediments were thrust up into large broken mountain masses, such as the Rigi, Rossberg, and other prominent heights along the northern flanks of the Alps. Since that last post-Eocene movement, no great orogenic paroxysm seems to have affected the Alpine region. But the chain has been left in a state of unstable equilibrium. From time to time normal faults have taken place whereby portions of the uplifted rocks have sunk down for hundreds of feet, and some of these dislocations have cut across the much older and more gigantic displacements of the thrust-planes (Fig. 282). At the same time continuous denudation has greatly transformed the surfaces of the ground, so that now cakes of gneiss are left as mountainous outliers upon a crushed and convoluted platform of Tertiary strata. Nor, in spite of the settling down of these broken masses, has final stability been attained. The frequent earthquakes of the Alpine region bear witness to the strain of the rocks underneath, and the relief from it obtained by occasional rents propagated through the crust along the length of the chain."

In view of the explanation of the folding of the Alps given in § 28, we need not comment on these views. They confirm the theory outlined in this paper, that the plications of all such chains must be sought in the actions of the sea, and mainly while the

range is under water, and not at all in the secular cooling of the globe.

V. Comparison of the Old and New Theory of Mountain Formation Continued.

(B) Views of Eminent Geologists on Mountain Formation in General.

§ 37. *Elie de Beaumont's Theory of the Secular Cooling and Collapse of the Globe.*—This venerable theory is thus condensed by Lyell:

"The origin of these chains depends not on partial volcanic action or a reiteration of ordinary earthquakes, but on the secular refrigeration of the entire planet. For the whole globe, with the exception of a thin envelope, much thinner in proportion than the shell to an egg, is a fused mass, kept fluid by heat, but constantly cooling and contracting in dimensions. The external crust does not gradually collapse and accommodate itself century after century to the shrunken nucleus, subsiding as often as there is a slight failure of support, but it is sustained throughout whole geological periods, so as to become partially separated from the nucleus until at last it gives way suddenly, cracking and falling in along determinate lines of fracture. During such a crisis the rocks are subjected to great lateral pressure, the unyielding ones are crushed, and the pliant strata bent, and are forced to pack themselves more closely into a smaller space, having no longer the same room to spread themselves out horizontally. At the same time, a large portion of the mass is squeezed upwards, because it is in the upward direction only that the excess in size of the envelope, as compared to the nucleus can find relief. This excess produces one or more of those folds or wrinkles in the earth's crust which we call mountain-chains."

De Beaumont's theory is given more from its antiquity than from its present day importance, and yet in some form it still holds its place in all our treaties on geology. Indeed the latest works include discussions of the strength of domes, as if the nucleus of the globe were shrinking away from the crust, and the latter thus subjected to crushing from its own weight.

§ 38. *Views of Lyell.*—This great geologist always rejected Elie de Beaumont's theories of mountain formation, and gave the most cogent reasons for his course. He adopted the theory that the land is occasionally depressed and elevated, by internal forces, but did not definitely decide what forces produced these progressive

or oscillatory movements of the earth's crust. One of Lyell's greatest disciples was Charles Darwin, whose views we shall now very briefly recall.

§ 39. *Views of Charles Darwin.*—The views of Darwin are very briefly and lucidly set forth by Professor Suess ("Face of the Earth," Vol. I, p. 104), as follows:

"The earthquake of February 20, 1835 (at Conception, Chili), gave rise to one of the most important works on the elevation of mountains, indeed I may say to the the only attempt, based on direct observation of nature, to establish more exactly the older theories concerning the force which is supposed to have raised up mountain chains. The author of this work is Charles Darwin. Since that time no second attempt, or at least no attempt of equal importance, has been made in this direction. To day, more than half a century later, it is possible to hold other opinions on these questions and yet to recognize the boldness of the generalization which even then revealed the master.

"Darwin saw the awakening activity of the volcanoes during and after the earthquake; he believed he saw elevation, although not uniform elevation of the solid ground; in addition he saw the terraces along the coast. But he also knew that similar terraces occur on the east coast of South America, where there are no volcanoes and no earthquakes. The earthquakes must therefore have appeared to his eyes as the local expression of a universal force. The secular contraction of the earth, a theory already eagerly advocated by several investigators, Darwin justly held to be entirely unsuited to explain those intermittent elevations which the terraces betrayed, and thus he reached the conclusion:

"'*That the form of the fluid surface of the nucleus of the earth is subject to some change, the cause of which is entirely unknown and the effect of which is slow, intermittent but irresistible.*'"

§ 40. *Views of Professor James D. Dana.*—The views of this eminent geologist have been carefully discussed in the paper on "The New Theory of Earthquakes and Mountain Formation as Illustrated by Processes Now at Work in the Depths of the Sea," § 13. The reader is referred to that discussion. Here it must suffice to say that, although Dana recognized that there was a fundamental relationship between the depth and extent of an ocean and the height of the mountains which surround it, he was unable to define this relationship except in very general terms, and could not assign any definite cause for the law which he pointed out. He considered the oceanic basins as subsiding, while the continents were being elevated.

Though Dana's views were somewhat modified by later study and investigation, he always maintained that "the principal mountain chains are portions of the earth's crust which have been pushed up and often crumpled or plicated by lateral pressure resulting from the earth's contraction." In order to explain this supposed mode of action he held that the oceanic areas have been "the regions of greatest contraction and subsidence, and that their sides have been pushed like the ends of an arch, against the borders of the continents."

Even with these arbitrary assumptions it is not at all clear how the settlement of the Pacific Ocean could elevate our great plateau west of the Rocky Mountains, which is nearly a thousand miles wide. If the subsidence of the ocean bed had pushed up the margin of North America, the crumpling and elevation of the land could not well extend one third of the way across the continent. We need not, however, be greatly surprised at this difficulty, for at best Dana's theory is vague, and he evidently could not understand just how the elevation had come about. Yet so fully was Dana convinced of the dependence of the mountains on the oceans adjacent to them that he reduced it to calculation by the rule-of-three. He says:

"The relation of the oceans to the mountain borders is so exact that the rule-of-three form of statement cannot be far from the truth. *As the size of the Appalachians to the size of the Atlantic, so is the size of the Rocky chain to the size of the Pacific.* Also, *as the height of the Rocky chain to the extent of the North Pacific, so are the height and boldness of the Andes to the extent of the South Pacific.*" ("Manual of Geology," 1863, p. 25.)

This was indeed a remarkably near approach to the great law of nature, that the mountains along the coasts are formed by the expulsion of lava from under the sea, and are, therefore, everywhere proportional to extent and depth of the adjacent oceans.

§ 41. *Views of James Hall.*—In 1857 this distinguished American geologist announced in a presidential address to the American Association at Montreal, that the enormous depth of the sedimentation along the Appalachian chain was due to the prolonged accumulation of sediments along a sinking, off-shore line of sea bottom. He reached this view from the careful study of the

Appalachian and other American mountain regions. To explain such deposits he supposed that marine currents had formerly traversed these regions and by gradually depositing sediments of great weight had also sunk the crust till at length a great thickness was attained. When the rocks thus formed had become solidified and crystallized the borders of the continent were afterwards upraised somehow. He did not indicate how the uplift had come about, nor did he think that the mountain regions had been raised separately. Denudation had then commenced, and finally given the mountains the forms they have today.

Keferstein, Sir John Herschel, Dr. T. Sterry Hunt and others, along with Hall, or even before him, in some cases, had developed the theory of aqueo-igneous fusion, which was supposed to produce a plastic zone between the consolidated crust and the solid nucleus. This theory supposed that the isogeotherms rise in regions of heavy sedimentation. Hall held that this would

"cause the bottom strata to establish lines of weakness or of least resistance in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself in those regions, and along those lines where the ocean's bed is subsiding beneath the accumulated sediments."

Many of the views afterwards more fully developed by Leconte are here faintly traced by Hall, and for that reason these early views of mountain formation are worthy of attention.

§ 42. *Views of Leconte.*—This veteran geologist gave great attention to mountain formation throughout a long career, and his residence on the Pacific Coast gave him exceptional facilities for studying the ranges of our western states, and especially of California, which includes the most remarkable developments in North America. The views at which Leconte arrived, as set forth in his "Elements of Geology," edition of 1896, are as follows:

"*Mountain Origin.*

"Leaving aside for the present all disputed points, it is now universally admitted that mountains are not usually pushed up by a vertical force from beneath, as once supposed, but are formed wholly by *lateral pressure.* The earth's crust along certain lines is *crushed together* by lateral or horizontal pressure and rises into a mountain-range along the line of yielding, and to a height proportionate to the amount of mashing. But the yielding is not by rising into a hollow arch, nor into such an arch filled beneath with liquid

(for in neither case would the arch support itself), but by mashing together and in thickening and crumpling of the strata and an upswelling of the whole mass along the line of greatest yielding. That this is the immediate or *proximate* cause of the origin or elevation of mountains is plainly shown by their structure. As to the *ultimate cause—i. e.*, the cause of the enormous lateral pressure—this lies still in the field of discussion. We shall discuss it briefly in its proper place" (pp. 261–2).

Again, on page 264, we find this account:

"*Proof of Elevation by Lateral Pressure alone: 1. Folding.*—It is evident that foldings such as those represented in all the above figures, and which occur in nearly all mountains, cannot be produced except by lateral pressure, and are therefore proof of such pressure. But, moreover, it can be shown that, when we take into consideration the immense thickness of mountain strata and the degree of folding, lateral pressure is *sufficient* to account for the whole elevation, without calling in the aid of any upward pushing from beneath. For example, the Coast range of California (Fig. 228) is composed of at least five anticlines and corresponding synclines. If

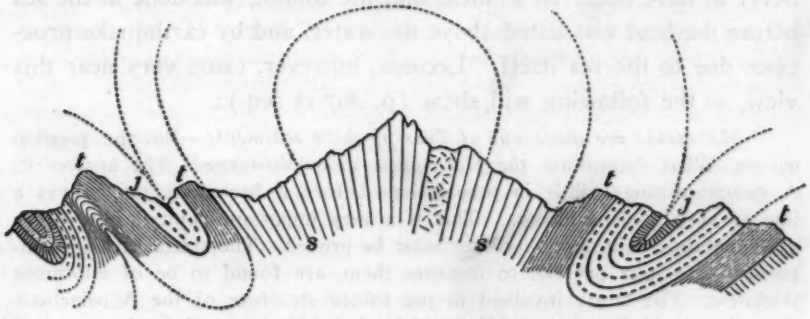

FIG. 14. Section of a Portion of the Alps.

its folded strata were spread out horizontally in the position of the original sediments, they would undoubtedly cover double the space. Now, supposing the strata here are only 10,000 feet thick—a very moderate estimate—in mashing to one half the extent, they would be thickened to 20,000 feet, which would be a clear elevation of 10,000 feet if they were not subsequently eroded. According to Renevier, a section of the Alps reveals seven anticlines and corresponding synclines, and some of them are complete overfolds (Fig. 230). We are safe in saying that Alpine strata have been mashed horizontally into one half their original extent. Supposing these were originally 30,000 feet thick (they were really much thicker), this would make a clear elevation of 30,000 feet. Of course, most of this has been cut away by erosion. In the Appalachian range, according to Claypole, the foldings are so extreme that in one place 95 miles of original extent have been mashed into 16 miles, or six into one, and yet the Appalachian strata are estimated as 40,000 feet thick. Cases of still greater doubling of strata upon themselves occur. In

the Highlands of Scotland the strata by lateral thrust were broken and slidden one over another for ten miles. In the Canadian Rocky Mountains there is an overthrust of seven miles, by which the Cambrian is made to override the Cretaceous, and 50 miles of strata are mashed into 25 miles (McConnell). In the Appalachians of Georgia the *Rome fault* is an overthrust which brings the Cambrian in contact with the Carboniferous and the fault under different names may be traced northward for 275 miles; and in the Cartersville thrust-fault there is an overriding of 11 miles (Hayes). The manner in which this is done is illustrated on a previous page (Fig. 209). Evidently, then, the whole height of the mountains mentioned above is due to lateral crushing alone."

If Professor Leconte had been familiar with the folding produced in the sea trenches he could have completed the theory of mountain formation developed in this paper. As geologists have for centuries recognized the fossils found in mountains as having been deposited in the sea, it is remarkable that the suggestion seems never to have occurred to them that the folding was done in the sea before the land was lifted above the water, and by earthquake processes due to the sea itself. Leconte, however, came very near this view, as the following will show (p. 267 et seq.):

"*Mountains are made out of lines of thick sediments.*—But the question occurs, What determines *the place* of a mountain-range? The answer is, A mountain-range while in preparation—before it became a range—was a line of very thick sediments. This is a very important point in the theory of mountain origin, and therefore must be proved. The strata of all mountains, where it is possible to measure them, are found to be of enormous thickness. The strata involved in the folded structure of the Appalachian, according to Hall, are 40,000 feet thick, the strata exposed in the structure of the Wahsatch, according to King, are more than 50,000 feet thick; the Cretaceous strata of the Coast Range, near the Bay of San Francisco, according to Whitney, are 20,000 feet thick; and if we add to this 10,000 feet for the Eocene and Miocene strata, the whole thickness is probably not less than 30,000 feet, while the Cretaceous alone in Northern California, according to Diller, is 30,000 feet. The Alpine geologists estimate the thickness of the strata involved in the intricate structure of the Alps as 50,000 feet. The strata of Uintah, according to Powell, are 32,000 feet thick.

"Now, it must not be imagined that these numbers merely represent the *general* thickness of the stratified crust; only that in these places the strata are turned up and their edges exposed by erosion, and thus their thickness revealed. On the contrary, it may be shown that the same strata are much thinner elsewhere. The same strata which along the Appalachian range are 40,000 feet thick, when traced westward thin out to 4,000 feet at the Mississippi River. The same strata which along the line of the Wah-

satch are 30,000 feet thick, when traced eastward thin out to 2,000 feet in the region of the plains. It is evident, therefore, that mountain-ranges are lines of *exceptionally* thick strata.

"*Mountain-ranges were once Marginal Sea-Bottoms.*—Where, then, do sediments now accumulate in greatest thickness? Evidently on marginal sea-bottoms, off the coasts of continents. The greater part of the washings of continents are deposited within 30 miles of shore, and the whole usually within 100 miles. From this line of thickest and coarsest deposit the sediments grow thinner and finer as we go seaward. But evidently such enormous thicknesses as 40,000 feet cannot accumulate in the same place *without pari passu subsidence* such as we know takes place now whenever exceptionally abundant sedimentation is going on (p. 145). Therefore, *mountain-ranges before they were yet born*—while still in preparation as embryos in the womb of the ocean—*were lines of thick off-shore deposits gradually subsiding*, and thus ever renewing the conditions of continuous deposits.

"As this is a very important point, it is necessary to stop here awhile in order to show that such was actually the fact in the case of all the principal ranges of the American Continent—*i. e.*, that for a long time before they were actually formed, the places which they now occupy were marginal sea-bottoms receiving abundant sediments from an adjacent continent. We shall be compelled to anticipate some things that belong to Part III, but we hope to make statements so general that there will be no difficulty in understanding them.

"1. *Appalachian.*—The history of this range is briefly as follows: At the beginning of the Palæozoic era there was a great V-shaped land-mass, occupying the region now covered by Labrador and Canada, then turning northwestward from Lake Superior and extending perhaps to polar regions about the mouth of the Mackenzie River. This is shown on map, Fig. 269, on page 303. There is another great land-mass occupying the present place of the eastern slope of the Blue Ridge and extending eastward probably far beyond the present limits of the continent—as shown in the same figure by dotted line in the Atlantic Ocean. The western coast-line of this land-mass was the present place of the Blue Ridge. Westward of this line extended a great ocean—'the interior Palæozoic Sea.' The Appalachian range west of the Blue Ridge was then the *marginal bottom of that sea.* During the whole of the Cambrian, Silurian, and Devonian, this shoreline remained nearly in the same place, although there was probably a slow transference westward. Meanwhile, throughout this immense period of time, the washings from the land-mass eastward accumulated along the shore-line, until 30,000 feet of thickness was attained. At the end of the Devonian some considerable changes of physical geography of this region took place, which we will explain when we come to treat of the history of this period. Suffice it to say now that during the Carboniferous the region of the Appalachian was sometimes above the sea as a coal-swamp, and sometimes below, but all the time receiving sediment until 9,000 or 10,000 feet more of thickness was added, and the aggregate thickness became

40,000 feet. Of course, it is impossible that such thickness could accumulate on the same spot without *pari passu* subsidence of the sea-floor. In fact, we have abundant evidences of comparatively shallow water at every step of the process—evidence sometimes in the character of the fossils, sometimes in the form of shore-marks of all kinds, sometimes in the form of seams of coal, showing even swamp-land conditions. Again, of course, the sediments were thickest and coarsest near the shore-line, and thinned out and became finer towards the open sea, *i. e.*, westward. Finally, after 40,000 feet of sediments had accumulated along this line the earth-crust in this region gave way to lateral pressure, and the sediments were mashed together and folded and swollen up into the Appalachian range. Subsequent erosion has sculptured it into the forms of scenic beauty which we find there to-day.

"2. *Sierra.*—This was apparently the *first-born* of the Cordilleran family. Its history is as follows: During the whole Palæozoic and earlier part of the Mesozoic, there was in the Basin region a land-mass, whose form and dimensions we yet imperfectly know, but whose Pacific shore-line was *east of the Sierra.* The Sierra region was therefore at that time the marginal bottom of the Pacific Ocean. Probably the position of this shore line changed considerably at the end of the Palæozoic. The extent of this change we will discuss hereafter. Suffice it to say now that, during the whole of this time, the Sierra region received sediments from this land-mass until an enormous thickness (how much we do not know, because the foldings are too complex to allow of estimate) was accumulated. At last at the end of the Jurassic, the sea floor gave way to the increasing lateral pressure along the line of thickest sediments, and these latter were crushed together with complex foldings and swollen up into the Sierra. An almost inconceivable subsequent erosion has sculptured it into the forms of beauty and grandeur which characterize its magnificent scenery.

"3. *Coast Range.*—The birth of the Sierra transferred the Pacific shore-line westward, and the waves now washed against the western foot of that range, or possibly even father westward in the region of the Sacramento and San Joaquin plains. At this time, therefore, the region of the Coast Range was the marginal bottom of the Pacific Ocean. During the whole Cretaceous, Eocene, and Miocene, this region received abundant sediments from the now greatly enlarged continental mass to the eastward; until finally, at the end of the Miocene, when 30,000 feet of sediments had accumulated along this line, the sea-floor yielded to the lateral pressure, and the Coast Range was born; and the coast-line transferred to near its present position.

"4. *Wahsatch.*—The physical geography of the region to the east of the Wahsatch (Plateau region) during Jura-Trias time is little known. But during the Cretaceous the region of the Wahsatch was the western marginal bottom of the great interior Cretaceous Sea (see map, Fig. 760, p. 486), receiving abundant sediments from the great land-mass of the Basin and Sierra region. This greatly increased the enormous thickness of sediments already accumulated along this line in earlier times. At the end of the Cretaceous the sediments yielded, and the Wahsatch was born. It is necessary, however, to say that both the Sierra and Wahsatch underwent very

great changes of form produced by a different process and at a much earlier period. We shall speak of this later.

"5. *Alps.*—Mr. Judd has recently shown that the region of the Alps, during the whole Mesozoic and Early Tertiary, was a marginal sea bottom, receiving sediments until a thickness was attained not less than that of the Appalachian strata. At the end of the Eocene these enormously thick sediments were crushed together with complicated foldings and swollen upward to form these mountains and afterward sculptured to their present forms.

"The same may be said of the Himalayas and nearly all other mountains. We may, therefore, confidently generalize, and say that the place now occupied by mountain-ranges have been previous to their formation, places of great sedimentation, and therefore usually marginal *ocean bottoms.* In some cases, however, the deposits in interior seas or mediterraneans have yielded in a similar way, giving rise to more irregular ranges or groups of mountains." . . .

"*Why thick Sediments should be Lines of Yielding.*—Admitting, then, that mountains are formed by the squeezing together of lines of very thick sediments, the question still occurs, *Why does the yielding take place along these lines in preference to any others?* This is a capital point in the theory of mountain formation. The answer is as follows: We have already seen (p. 231) that accumulation of sediments causes the isogeotherm to rise and the interior heat of the earth to invade the lower portion of the sediments with their included waters. Now this invasion of heat in its turn causes hydrothermal softening or even fusion, not only of the sediments, but also of the sea-floor on which they rest. Thus a line of thick sediments becomes a line of softening and therefore a line of weakness, and a line of yielding to the lateral pressure, and therefore a line of mashing together and folding and upswelling—in other words a mountain-range. As soon as the yielding commences we have an additional source of heat in the crushing itself. In addition to this, upheaval by lateral crush by the tendency to arch the strata would produce relief of gravitative pressure, and therefore fusion (p. 103). It follows from this that there is or was beneath every mountain a line of fused or semi-fused matter. This we will call the *sub-mountain liquid.* This by cooling and solidification becomes a *metamorphic or granitic core,* which by erosion forms the metamorphic or granitic axis and crest of many great mountains" . . . (pp. 271–2).

"*Cause of Lateral Pressure.*—We have thus proved that the immediate cause of the origin and the growth of mountains is lateral pressure acting on thick sediments, crushing them together and swelling them up along the line of great thickness. But still the question remains, What is the *ultimate* cause, *i. e.,* the *cause of the lateral pressure?* This, as we have already said, lies still in the domain of doubt and discussion, but the view which seems most probable may be briefly stated as follows:

"In the secular cooling of the earth there would be not only unequal radial contraction, giving rise, as shown on page 175, to continents and ocean-basins, but also to unequal contraction of the *exterior* as compared with the *interior.* At first, and for a long time, the exterior would cool

fastest; but there would inevitably, sooner or later, come a time when the exterior, receiving heat from abroad (sun and space), as well as from within, would assume an almost constant temperature, while the interior would still continue to cool, and contract. Thus, therefore, after a while the interior nucleus would contract faster than the exterior shell. It would do so, partly because it would cool faster, and partly because the coefficient of contraction of a hot body is greater than that of a cooler body. Now, as soon as this condition was reached, the exterior shell, following down the shrinking nucleus, would be thrust upon itself by a lateral or horizontal pressure which would be simply irresistible. If the earth's crust were a hundred times more rigid than it is (thirty times as rigid as steel, 500 to 1,000 times as rigid as granite—Woodward, *Science*, Vol. XIV, p. 167, 1889), it must yield. Mountain-ranges are the lines along which the yielding takes place, and this yielding takes place along the lines of thick sediments because these are lines of weakness.

"There are several serious objections which may be brought against this view: 1. Calculations seem to show that the amount of crumpling and folding actually found in the mountains is many times greater than could be produced by the contraction of the earth by *cooling*. But it may be answered (1) that the calculations take no account of the greater coefficient of contraction at high temperatures, and therefore at great depths, (2) and that there may be *other causes* of contraction besides cooling. For example, loss of constituent gases and vapors from the interior of the earth, through volcanic vents and fissures, has been suggested by O. Fisher (p. 102).

"2. Again, it has been shown by Dutton that it is impossible that the effects of differential contraction should be concentrated along certain lines, so as to give rise to mountain-ranges without a shearing of the crust upon the interior portions, which is inadmissible if the earth be solid. Instead, therefore, of conspicuous mountain-ranges, the effects of differential contraction would be distributed all over the surface, and be wholly imperceptible. But in answer to this it may be said that there is no difficulty in the way of shearing, and therefore of such concentration of effects along certain lines, if *there be a sub-crust liquid* or semi-liquid layer, either universal or else underlying large areas of surface.

"Still other objections have been raised, but these are so recent that they have not yet been sufficiently sifted by discussion to deserve mention here.[1] The origin of mountains by lateral pressure is a fact beyond dispute. This is the most important fact for the geologist. How the lateral pressure is produced is a pure physical question which must be left to the physicists to settle among themselves" (pp. 274–5).

Leconte treats also of Monoclinal mountains, as found in the Great Basin, which he explains by normal faulting, or vertical movement of crust blocks, and finally adds:

[1] For a completer discussion of this subject, see "Theories of Mountain Origin," *Jour. Geol.*, Vol. I., p. 542, 1893.

"Thus, then, there are two types of mountains strongly contrasted, mountains of the one type are formed by lateral *pressure and crushing*, of the other type by lateral *tension and stretching*. The one gives rise mainly to reverse faults, the other always to normal faults. Mountains of the one type are formed by upswelling of thick sediments, those of the other type by irregular readjustment of crust-blocks. Mountains of the one type are *born of the sea*, those of the other type are born *on the land*. We find examples of the one type in nearly all the greatest mountains everywhere, but especially in the Appalachian, the Alps and the Coast Range. The best examples, perhaps the only examples, of the other type are the Basin ranges. Some mountains, as the Sierra, the Wahsatch, and certainly *some* of the Basin ranges, belong to both types. In their origin, they have formed in the first way, but afterward have been modified by the second way. Thus the first is the fundamental method, and the second only a modifying process" (p. 277).

These views of Leconte call for no special comment, beyond the remark that normal faulting itself is wholly unexplained. If secular cooling were the cause, such faults ought to occur east of the Rocky Mountains as well as west of them. The important difference is that the Pacific Ocean was on the west pushing up the land, and a continental basin on the east, either dry or covered by shallow water and therefore doing little or no pushing at all. In any case the great plateaus of the west were certainly uplifted by the Pacific, through the expulsion of lava under the land. In the Andes of South America the plateaus are higher indeed, but also narrower than those in North America, because in our continent the relief resulting from the leakage of the ocean took a broader and less elevated form. It is impossible for any one to doubt the identity of the forces which raised the Andes and their plateaus, the Himalayas and their plateaus, and the Rocky Mountains and the mountains and plateaus of the Great Basin. The principle of continuity shows clearly that the cause was everywhere one and the same. Several American geologists have suggested vertical uplifts in the Great Basin, from the way in which the crust blocks are displaced; but heretofore no known cause for such movements could be assigned, because it was held that secular cooling is the chief if not the only cause operating in the development of the globe.

§ 43. *Views of Rev. O. Fisher.*—The Rev. O. Fisher was the first to show by long and patient research the total inadequacy of secular cooling to account for the observed height of mountains.

He showed that the mountains are hundreds of times higher than the cooling of the earth will explain. On this point his labors mark a distinct advance in geological science; for next in importance to establishing true theories is the overthrow of erroneous ones, which clears the ground for a fresh start. But notwithstanding the unanswerable character of Fisher's argument, the old theories have been retained by geologists as the best they could devise. Fisher's criticisms of geological theories are carefully thought out, and worthy of attention. He has always denied the entire solidity of the earth, holding that the movements noticed in mountains proved the existence of a mobile substratum beneath a crust some twenty miles thick. Here again he was certainly right, and it is difficult to see how such an obvious proposition could be denied.

We need not dwell on Fisher's views of mountain formation, because they imply convection currents within the earth, and these latter are certainly inadmissible, except just beneath the crust in earthquake movements, as developed in the theory set forth in this paper.

§ 44. *Views of Major C. E. Dutton.*—Like the Rev. O. Fisher, Major Dutton was one of the earliest authorities to question the adequacy of secular cooling to account for the wrinklings noticed in the earth's crust. Using the results of Fourier's solution for the variation of temperature, as developed in the work of Lord Kelvin, Dutton found that

"the greatest possible contraction due to secular cooling is insufficient in amount to account for the phenomena attributed to it by the contraction hypothesis. By far the larger portion of this contraction must have taken place before the commencement of the Palæozoic age. By far the larger portion of the residue must have occurred before the beginning of the Tertiary, and yet the whole of this contraction would not be sufficient to account for the disturbances which have occurred since the close of the Cretaceous."

Major Dutton concludes that "the determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated." He held that the localization of the plications could result only from a large amount of horizontal slipping of the crust over the nucleus, and the friction involved in this movement even over a liquid nucleus would be so great as to render the assumption a physical absurdity.

If wrinkling resulted from uniform cooling and consequently uniform shrinkage, the effect would be analogous to that of a withered apple, with small wrinkles all over it, instead of a surface presenting in one region a continuous system of folds extending from Cape Horn to Alaska, and in another, a zone a thousand miles wide, from the Appalachian to the Rocky Mountains, with scarcely any evidence of disturbance whatever.

In these considerations Major Dutton has forcibly expressed the difficulty of supposing that a mountain range is formed by the cooling of the earth contracting equally along all its radii. Such a supposed mode of formation of our ranges, folded and crumpled as they are, is clearly impossible; and Major Dutton shares with the Rev. O. Fisher the credit of having been the first to recognize the total inadequacy of the contraction theory.

It is remarkable that after this antiquated theory had been thus clearly disproved, it should have continued in use. No one seems to have been able to frame a theory based on any cause except secular cooling, till the present writer developed the theory based on the leakage of the oceans and the formation of mountains by the expulsion of lava under the land, which perfectly explains all the phenomena.

§ 45. *Views of Geikie.*—In the article "Geology," *Encyclopedia Britannica*, p. 375, we find the following statement of the contraction theory:

"There still remains the problem to account for the original wrinkling of the surface of the globe, whereby the present great ridges and hollows were produced.

"It is now generally agreed that these inequalities have been produced by unequal contraction of the earth's mass, the interior contracting more than the outer crust, which must therefore have accommodated itself to this diminution of diameter by undergoing corrugation. But there seems to have been some original distribution of materials in the globe that initiated the depressions on the areas which they have retained. It has been already pointed out (ante, p. 223) that the matter underlying the oceans is more dense than that beneath the continents, and that, partly at least, to this cause must the present position of the oceans be attributed. The early and persistent subsidences of these areas, with the consequent increase of density, seems to have determined the main contours of the earth's surface. . . .

"The effects of this lateral pressure may show themselves either in broad dome-like elevations, or in narrower and loftier ridges of mountains.

The structure of the crust is so complex, and the resistance offered by it to the pressure is consequently so varied, that abundant cause is furnished for almost any diversity in the forms and distribution of the wrinkles into which it is thrown. It is evident, however, that the folds have tended to follow a linear direction. In North America, from early geological times, they have kept on the whole on the lines of meridians. In the Old World, on the contrary, they have chosen diverse trends, but the last great crumplings —those of the Alps, Caucasus, and the great mountain ranges of central Asia—have risen along parallels of latitude.

"Mountain chains must therefore be regarded as evidence of the shrinkage of the earth's mass. They may be the result of one movement, or of a long succession of such movements. Formed on lines of weakness in the crust, they have again and again given relief from the strain of compression by undergoing fresh crumpling and upheaval."

Geikie's views may be considered the accepted views of geologists generally, and it will be seen that they rest on the theory of contraction due to secular cooling.

On the constitution of the globe Geikie quotes ("Geology," p. 73) from the paper of Arrhenius, "Zur Physik des Vulcanismus" (1900), the following theory of the illustrious Swedish physicist:

"If the rocks at the earth's surface have a density half that of the globe as a whole, and if the density continues to hold good for the magma that arises from the melting of these rocks, we must conceive the existence of a much denser substance in the earth's interior. On various grounds, such as the preponderance of iron in nature, both in meteorites and in the sun, and the phenomena of terrestrial magnetism, it may be inferred that this substance is metallic iron. In consequence of its greater density this iron will naturally be deeper than the rock magma, and on account of the high temperature must exist in a gaseous condition. Somewhere about a half of the planet therefore should consequently consist of iron, and of other metals mingled with it in smaller proportions. The semi-diameter of this gaseous iron-sphere will thus include about 80 per cent. of the earth's semi-diameter. Then will come about 15 per cent. of the gaseous rock magma, next to it the liquid rock-magma for a thickness of about 4 per cent. of the terrestrial semi-diameter, and lastly the solid crust, for which not more than 1 per cent. may be claimed" (pp. 404–5).

Referring to the light thrown on the constitution of the interior by the observation of waves propagated by earthquakes, Geikie also adopts the theory of Arrhenius, which is as follows:

"The density of much the largest part (reckoned linearly) of this interior, amounting, as above stated, to about 80 per cent. of the radius, must be nearly three times higher than that of quartz. Since now the mean velocity of transmission of earthquake waves in the interior of the

earth has been ascertained to amount to 11.3 kilometers per second, the compressibility of that region must be 31 times less than that of quartz, that is, eight times less than that of solid steel, according to Voigt. This is a figure of precisely that order of magnitude which was to be expected. We may well believe that at depths of more than 1,000 kilometers the compressibility of gaseous iron sinks down to some ten times less than that of steel.

"The interior of the earth, therefore, with the exception of a solid crust about 40 kilometers thick, consists of a molten magma 100 or 200 kilometers in depth which shades continuously inward into a gaseous center. The liquids and gases in the interior possess a viscosity and incompressibility such as permit them to be regarded as solid bodies. From these, however, they are distinguished in the first place by the fact that differentiations are possible to a considerable degree, the effects of which may long endure. In the second place, long continued pressures, when acting on a large enough scale, may produce great deformations. Further, the liquids must possess the property of great expansion on a diminution of the high pressure, thereby readily becoming fluid. The process must thus differ but little from a normal melting with increase of volume, and especially of fluidity, as well as with absorption of heat. And yet the condition of aggregation is not thereby altered."

Geikie remarks that the theory of Arrhenius accords well with geological requirements:

"With reference to the crust of the earth, it meets the constantly repeated objections of the geologists to whom the existence of a comparatively thin crust has always seemed an essential condition for the production of that crumpled and fractured structure which the rocks of the land so universally present. If the solid crust of the earth is allowed to be about 25 miles thick, we must conceive that in the lower four fifths of its mass the rocks are in a condition of latent plasticity. They lie much beyond the crushing strength which they exhibit at the surafec. They are not crushed into powder as they would be under a similar strain above ground, but they are ready to yield to the deformations that may arise consequent upon adjustments of the gigantic pressure to which they are subjected. Hence the solid crust down as far as its structure has been disclosed abounds in proofs that it has undergone colossal plication and fracture, and that higher portions of it many square miles in extent have been thrust bodily over each other for many miles."

The last view here expressed by Geikie as to how the crust becomes thrust over itself for many miles is not, we think, well founded, because it is shown in this paper that all this folding and overlapping of the crust arises in the trenches dug out in the sea bottom by earthquakes. This crumpling and overthrusting of the crust certainly would not arise except for earthquakes produced by

the leakage of the oceans, to which mountain formation is due. Of course the plasticity of this layer beneath the crust contributes to the final result, but the leakage of the oceans, with the resulting earthquakes, supplies the deforming force.

§ 46. *Views of Professor Suess.*—In the "Face of the Earth" (Vol. I, p. 107) we find the following brief exposition of Professor Suess' views:

"*The dislocations visible in the rocky crust of the earth are the result of movements which are produced by a decrease in the volume of our planet.* The tensions resulting from this process show a tendency to resolve themselves into *tangential* and *radial* components, and thus into horizontal (*i. e.*, thrusting and folding), and into vertical (*i. e.*, sinking) movements. Dislocations may therefore be divided into two main groups, of which one is produced by the more or less horizontal, the other by the more or less vertical relative displacement of larger or smaller portions of the earth's crust.

"There are large areas in which the first, and others in which the second group predominates, and there are also regions in which both groups appear together, and in which an intimate connection may be recognized between them, the resolution of the movements in space having in these cases been less complete. This essential difference in the movements of the lithosphere may be clearly perceived from a comparative study of the structure of the Old World; nor has it escaped the notice of American geologists.

"'The geological provinces of the Great Basin,' remarks Clarence King, has suffered two different types of dynamic action: one in which the chief factor was evidently tangential compression, which resulted in contraction and plication, presumably in post-Jurassic time; the other of strictly vertical action, presumably within the Tertiary, in which there are few evidences or traces of tangential compression.'

"Our colleagues on the other side of the ocean have even gone a great deal further. After comparison of the folded Appalachian mountains with the depressed Basin Ranges, Gilbert had in 1875 already suggested the possibility that in the Appalachians the causes of movement were superficial, in the Basin Ranges deep-seated. We shall have an opportunity, when discussing the relation of the Alps to their northern foreland, of determining to what extent this supposition finds confirmation in Europe. We may however state at once that as a rule it is only the dislocations of the second group which are accompanied by volcanic eruptions."

§ 47. *Views of Arrhenius.*—It is well known that this distinguished Swedish physicist holds that the earth's interior is essentially gaseous (cf. § 45, above), but under the great pressure operating in the globe made to behave very nearly as a solid.[1] In his

[1] See Postscript, page 274.

paper "Zur Physik des Vulcanismus," published in 1900, Arrhenius points out that in fluids at high temperature, where no increase in volume takes place, the internal friction of the molecules rises with the temperature, so that the viscosity increases and the fluidity diminishes; that a similar effect is observable in both gases and liquids; that although gases have the highest and solids the lowest compressibility, nevertheless when a gas near its critical temperature passes into a liquid, through a trifling physical change, there is practically no change in the compressibility. The higher the pressure the smaller is the compressibility, and a gas above the critical temperature may be made to acquire the properties of a solid by pressure alone. Such a mass has great density, small compressibility, and large viscosity, so that it has the properties of a solid, though really an imprisoned gas.

At a depth of 40 kilometers Arrhenius says the temperature is about 1200° C., and the pressure about 10,840 atmospheres; and as these conditions would render nearly all ordinary minerals fluid, he concludes that below that depth the matter is molten, in the form of a magma—that is, a viscous and nearly incompressible liquid made to act nearly as a solid by pressure.

At greater depths the temperature is above the critical temperature of every known substance, as the pressure rapidly increases and the liquid magma becomes a gaseous magma with larger and larger viscosity, and smaller and smaller compressibility—in other words, an elastic solid with rigidity increasing with the depth.

VI. Abandonment of the Old Theories of the Physics of the Earth.

§ 48. *The Total Inadequacy of the Old Theories to Account for the Fault Movements near the Sea, which Raise Vertical Blocks and Walls of Granite*[1] *Thousands of Feet above the Water.*—The vast

[1] Andesite is the name used to designate the kind of granitic rock found in the Andes. Charles Darwin showed that all granitic rocks are closely related. In his "Text-book of Geology," edition of 1903, book II, Part II, § 7, pp. 230–260, Sir Archibald Geikie gives tables of the chemical compositions of all these rocks, which show very clearly their close relationship. When we use the term *granite* therefore we mean *granitic rock* in the wide sense.

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vertical walls and blocks of granite so often lifted thousands of feet above the sea, with deep water all around their bases, frequently encountered in different parts of the world, cannot be explained except by the present theory. Thus along the west coast of Chili and Patagonia, from Cape Horn to Valparaiso, in the Straits of Magellan, as well as in the ranges of the Andes further from the coast, in the Sierras of California, and elsewhere these vertical uplifts are common. It is obvious that they cannot possibly be explained by the old theories depending on the shrinkage of the globe. But if lava is expelled from beneath the sea, owing to the secular leakage of the ocean bottom, and the crust is fractured and rent into blocks by the earthquake forces, some of these blocks would naturally be pushed upward, leaving vertical walls of granite thousands of feet high. Occasionally the blocks would be forced apart, leaving the sea pass between, as so often seen in Chili, Patagonia and Tierra Del Fuego. The Straits of Magellan no doubt arose in this way. As already remarked in § 27, Darwin describes similar breaks in the Andes further north, through which the sea once flowed, but they are now raised above the water. No doubt the time will come when Tierra Del Fuego will be joined solid to Patagonia, by uplifts which will cause the sea to withdraw from the Straits of Magellan and it will become dry land, like those ancient passages further north mentioned by Darwin.

There are many other parts of the world where similar phenomena may be seen. The origin of the fiords in Norway has long been a matter of debate. It seems to be conceded that these inlets are made by mountains running into the sea, and more or less modified above water by ice and glaciers. They are supposed to be quite old, and certainly date back of the glacial epochs.

It may no doubt be safely assumed that these Norwegian mountains originated, like other mountains, by the uplift of faults, owing to the expulsion of lava from beneath the sea.[1] Hence the precipitious walls along the sea coast, with deep water between. The blocks

[1] Having read the earlier papers of this series with great interest, Professor Schiaparelli has kindly called my attention to the trough in the sea along the Norwegian coast. This confirmation of the theory by the illustrious astronomer of Milan is exceedingly interesting.

of the earth's crust were lifted vertically by the pushing of lava beneath them. It is in this way that all such walls of granite and other towering rock are to be explained, and the fact that the sea still encroaches on them shows how the movements came about. Probably there has been little vertical movement for a long time along the coast of Norway, and subsidence as well as elevation may have taken place, both here and elsewhere. Subsidence is common along most sea coasts, but it does not prevail in the long run, as is proved by Professor Suess's work, showing a universal lowering of the strand line throughout the world.

§ 49. *The Theory of Arches and Domes Inapplicable to the Crust of the Earth, because the Globe is not Shrinking but actually Expanding.*—In Chamberlin and Salisbury's "Geology," Vol. I, p. 583, we find the statement that

> "The principle of the dome is brought into play whenever an interior shell shrinks away, or tends to shrink away, from an outer one which does not shrink. In this case there is a free outer surface and a more or less unsupported under surface towards which motion is possible. The dome may, therefore, yield by crushing or by contortion."

Owing to the important part the domed form of the crust has played in theories of deformation, these authors give quantitive results calculated by Hoskins, showing that such a dome of continental dimensions, if unsupported from below, *would sustain only 1/525th of its own weight.*

In his consideration of the "Mathematical Theories of the Earth" (*Proc. Am. Assoc. for Adv. Sci.*, 1889, p. 49), Professor R. S. Woodward reached the analogous conclusion that "If the crust of the earth were self-supporting, its crushing strength would have to be about thirty times that of the best cast steel, or five hundred to one thousand times that of granite."

In view of these results it is remarkable that any one should have viewed the earth's crust as a wholly or partially self-supporting dome; for it could not be supported even over a very small area. And moreover secular cooling is wholly inadequate to cause a separation of the interior layers from the crust. All that has been published on this point, therefore, is inapplicable to the earth, because it rests on a false hypothesis. The supposed conditions have no reality.

The earth is not shrinking and the crust does not tend to separate itself from the underlayers, except where the lava has been expelled from beneath it by earthquakes. The collapse of the crust when thus undermined, however, shows that it will not support its own weight even for a short distance. Over such small areas the crust may be taken as part of a plane, or sometimes as concave, where subsidence is already at work, and hence the theory of the arch or dome is scarcely applicable; yet the observed collapse and sinking, even where the area is no larger than in ocean troughs, confirms the above conclusions regarding the total inability of the crust to support itself.

Could therefore anything be more absurd than to discuss the stresses in the crust due to the progress of secular cooling? Stresses arise only where mountain making is in progress, and therefore chiefly near the oceans, but never appear far inland; and are wholly due to the pressure arising from steam-saturated rock and the expulsion of lava from beneath the oceans, or to movements traceable to surface water slowly sinking into the earth. The theory of arches and domes therefore confirms the present theory, but this result is indirect; and such lines of thought did not enable geologists and physicists to reach correct conceptions regarding the physics of the earth's crust.

§ 50. *On the Doctrine that Earthquake Movements depend on Slight Inequalities of Loading, and on the Abandoned Theory that the Earth is a Failing Structure.*—As the crust of the earth is made up of solid rock and soil arising from the disintegration of rock of various kinds, and as this material is elastic and yields under pressure, it naturally occurred to physicists that inequalities of surface loading deposited on adjacent areas would impose upon the underlying crust unequal stresses, and perhaps give rise to relative movements. Thus many physicists, in default of a better theory, have supposed that surface loads, depending on erosion and sedimentation, tides and varying barometric pressure, would be adequate to produce stresses that would cause readjustment of the surface strata and perhaps movements of faults in earthquakes.

It is undeniable that these varying loads do produce some small effects, and very slight changes of level may often arise in this

way. We owe the establishment of these effects of loading chiefly to the researches of Professor Sir G. H. Darwin, whose labors have so greatly advanced our knowledge of the physics of the earth. They have an extremely high importance in the theory of bodies approximating elastic solids. The undisturbed crust of the globe fulfills these conditions quite perfectly.

But to suppose that any of these small surface effects could give rise to world-shaking earthquakes which would shake down cities, raise sea coasts, and uplift mountains and islands in the sea, is too severe a test of credulity to be entertained. The class *of minute movements,* due to surface yielding under varying loads depending on sediments, tides and meteorological causes, and the class of *great movements,* due to the expulsion of lava from under the bed of the sea, are quite distinct. One class of these phenomena is microseismic, the other magaseismic. Previous investigators have generally confounded the two classes of phenomena, and hence they have been unable to recognize the true cause of earthquakes and mountain formation. For that reason it was necessary to restrict our investigation to the great disturbances, in the first search for the cause of the great movements of the earth's crust.

We repeat that both classes of phenomena are important in a complete theory of the physics of the earth; but the small yieldings of microscopic dimensions must be kept distinct from the great movements which have shaped the surface of the globe. Many of the small effects depend on the greater movements of the earth, while few of the great movements are influenced by surface forces—indeed none at all, except where accumulation of subterranean stresses has already rendered the conditions highly unstable. In this latter case small surface forces may occasionally accelerate the outbreak of an earthquake, just as a spark discharges a loaded gun, or a shock explodes a charge of dynamite.

On a par with the theory that slight inequalities of surface loading produce earthquakes is another equally untenable view that the earth is a failing structure. Such a doctrine might have been entertained a quarter of a century ago, when the theory of secular cooling was generally accepted, but to-day such a view is antiquated and utterly indefensible. *Owing to the demonstrated de-*

pendence of mountain making upon the sea the earth emphatically is not a failing structure. So far from failing by collapse, our planet seems to be expanding from 10 to 100 faster than it contracts from loss of heat. Thus have arisen all the highest mountains and plateaus of the globe. These great uplifts invariably face the deepest oceans, from which the expulsion of lava has mainly proceeded. Such antiquated doctrines as that the earth is a failing structure are now absolutely without excuse, and practically abandoned, and the sooner they disappear from scientific literature the better for sound knowledge of the physics of the earth.

§ 51. *Changes of the Force of Gravity in Regions Affected by the Movement of Lava Beneath the Crust.*—In view of the demonstrated movement of lava streams beneath the crust of the globe, it follows that such bodily displacement of matter but a short distance below the surface may modify sensibly the observed intensity of gravity. A region which is being undermined will have the intensity of gravity decreased, and a region which is being filled up will have the attraction increased. And not only will the *intensity* vary, but also the *direction* of the vertical, according to the movements which occur beneath the crust. And these effects may be large enough to become sensible to very refined observation.

It is in this way that the anomalies of gravity in the neighborhood of mountains have arisen in the process of mountain formation. And in regions where the expulsion of lava is still in progress, both the direction and intensity of gravity are subject to change by earthquakes. Thus in the region of the Aleutian Islands, the east coast of Japan, and many other places, such as the west coast of South America, the direction and intensity of gravity is certainly subject to change by seismic disturbances.

As the crust of the globe often suffers horizontal and vertical movement during the greatest earthquakes, the *altitude* and *azimuth* of places are also subject to change; and exact geodetic triangulation remains valid only for the interval between great earthquakes. Even then there may be a very slow and gradual settlement owing to plastic yielding of the crust and especially of the substratum beneath. Thus after earthquakes such as occur in Peru and Chili, Japan and Alaska, gravity and geodetic determinations need repeti-

tion, as was done in California after the great earthquake of April 18, 1906. And as the disturbance may alter the direction and intensity of local gravity, this possibility must be taken account of in the repetition of the observations. In order to be entirely rigorous the equations connecting the triangulation should include undetermined multipliers to take account of possible variations in the local attraction at each point. If with this general condition imposed, the triangulation before and after the earthquake comes out rigorously the same, within the limits of errors of observation, it may be supposed that the surface effects of the disturbance are insensible; otherwise the difference must be attributed to disturbances due to the earthquake.

With the refinement now possible in geodesy, it is not to be doubted that these effects will occasionally prove to be sensible to observation. The great earthquake in Assam-Bengal gave rise to horizontal movements of the order of 20 or 30 feet, which may affect the latitude by 0″.2 or 0″.3, and are thus within the limits of astronomical measurement. But apparent changes in latitude may result from change in the direction of gravity as well as from actual displacements of the crust, and both possibilities need to be taken into account.

§ 52. *The Necessity of Further Study of the Contours and Movements of the Sea Bottom.*—In view of the results brought out in this paper and those which have preceded it, but especially that on "The New Theory of Earthquakes and Mountain Formation as Illustrated by Processes now at Work in the Depths of the Sea," it is scarcely necessary to point out the extreme importance of further study of the contours and movements of the sea bottom. Our present maps of the ocean depths are very incomplete, although they afford a good general idea of the sea basins. But one can scarcely doubt that more exact surveys would bring to light additional mountain ranges and plateaus in regions heretofore but slightly explored; moreover certain places in the sea bottom would be found to be covered with a great variety of peaks or submerged islands which do not reach the surface.

Where the water is deep the exact survey of the bottom presents considerable difficulty. As movements arising from earth-

quakes are extremely small in comparison with the depth of the sea, it would perhaps be very difficult to detect resulting changes of the sea bottom, except in cases where sinking takes place, and the drop is large. In some cases of actual measurement in the laying of cables the sinking has been found to be hundreds of fathoms, which would be very easily recognized if the exact place of former soundings could be found. But as the changes of level in the sea bottom are fully as capricious as on land, we see that regions where mountain formation is in progress would present extreme complexity; and unless the place were very accurately known, one could not be sure that two soundings were over the same spot. This difficulty would be less near known islands than in the open sea, but it would be considerable in all places where the ship is at the mercy of the winds and currents.

Under the circumstances it is clear that great natural difficulty would arise in the exact Hydrographic survey of the deep sea, and an economic difficulty would be added, on the ground that such surveys are not required in practical navigation. Yet the laying and repair of cables would necessitate fairly accurate knowledge of the depths, and we may hope, in spite of the growth of the wireless telegraph, that our ocean surveys are still in the infancy of what they will be in another half century.

Where trenches are being dug out by earthquakes there will be the double incentive to ascertain the stage of the process and the rapidity and location of the changes. These considerations may contribute to our knowledge of particular regions; and, after all, the changes in the larger regions of the ocean bottom are small.

When the regions in which trenches are forming are once clearly recognized, attention will naturally be centered upon them, to the neglect of less disturbed areas. The most interesting regions, from a seismological point of view, are those in which islands are being uplifted and the sea bottom sinking, as near the Aleutian, Kurile and Japanese islands, the Antandes, and along the west coast of South America. But it may also be hoped that the changes in depth near individual islands, such as Guam and Martinique, will not be overlooked. Here the subsidence of the bottom often takes the form of a hole rather than of a trench. Yet

in time the movements may give rise to neighboring islands. All of these considerations show the value of accurate knowledge of the sea bottom at this epoch.

§ 53. *Greatness of the Forces which Uplift and fold the Earth's Crust.*—The tremendous power of earthquake and volcanic forces has been proverbial from the earliest ages of history, and finds expression also in the universal terror thus excited among all living beings. This extreme terror is only too well justified by the vast extent of the ruin too often wrought in different parts of the world. But probably only those who have witnessed a great earthquake can adequately appreciate the awful character of the commotion, and the gigantic forces which must underly it. This is shown also by the many published attempts to belittle the significance of earthquake disasters.

Some writers of eminent mathematical learning, but apparently lacking in grasp of the larger physical phenomena, have ascribed earthquakes to inequalities of loading, changes of barometric pressure, etc., and have with strange and almost marvelous credulity believed that the settlements of the earth thus arising would shake down cities and devastate whole countries. How these learned authorities imagined that small subsidences under the steady action of these infinitesimal forces could bring about such long continued shaking and proportionately great havoc is difficult to understand. If the forces are so small, and act so slowly, is it conceivable that the yielding could be anything else than gradual and insensible? Such minute settlements evidently would be like those now experienced in dry inland regions free from real earthquakes.

The titanic nature of the forces which have uplifted islands, mountains, plateaus and continents, can scarcely be realized; yet even the ancients grasped it to some extent when they described the whole region between Naples and Sicily as underlaid by a giant, whose movements disturbed the intervening sea bottom. In his account of the Chilean earthquake of 1835, Charles Darwin showed that the entire region from the island of San Fernandez to the Andes, about 450 miles across, had been moved together by underlying forces. "There was undoubtedly a connection between the

volcanic forces acting under this island, and under the continent, as was shown during the earthquake of 1835," says the great naturalist.

As such views have been carefully set forth by the greatest of original investigators, from Aristotle to Darwin, it is remarkable to witness the puny efforts which have been made to belittle these forces. A gentleman holding a university position, in a public address at Boston, recently likened the shock of an earthquake to the jar experienced by an insect attached to a reed which was bent till it snapped. According to this authority the earthquakes are due to the snapping of the rock of the earth's crust in the bending produced by secular cooling. Is it necessary to point out the misleading character of the comparison made, and this lecturer's utter inability to grasp the phenomena of nature?

An equally common fallacy is to ascribe these tremendous disturbances to inequalities of surface loading, due to geological and meterological causes. Such views seem the more surprising, because formerly they have proceeded from physicists of eminent learning. But at least partial excuse may be found in the universal acceptance of the theory of secular cooling heretofore, and in the proved rigidity of the globe, which naturally led to the supposition that the crust was adjusting itself to the shrinking sphere.

Before the development of the theory of ocean leakage no adequate theory presented itself to investigators, who had unfortunately not discriminated between the great and small earthquakes. With a false premise and such an indiscriminate mixture of phenomena, real progress was difficult, if not impossible.

§ 54. *Darwin's Remarks on the Forces which Uplift Continents.* —In the extract quoted from Professor Suess, § 39, allusion has already been made to Charles Darwin's attempt to explain the origin of mountains by the direct observation of nature. His paper "On the Connection of Certain Volcanic Phenomena in South America and the Formation of Mountain Chains and Volcanoes as the Effect of the Same Power by which Continents are Elevated" (*Transactions of the Geological Society*, Vol. V, 1838, pp. 601–631) led Darwin to the conclusion:

> *"That the form of the fluid surface of the nucleus of the earth is subject to some change, the cause of which is entirely unknown and the effect of which is slow, intermittent, but irresistible."*

Again, in the "Voyage of the Naturalist," Chapter XIV, he adds:

"The forces which slowly and by little starts uplift continents, and those which at successive periods pour forth volcanic matter from open orifices, are identical."

It is unnecessary to dwell on the irresistible power which the great naturalist correctly abscribed to volcanic and earthquake forces. It is of more interest to notice that he declared them to be identical with those which uplift continents. The same result is reached in the present paper, about three quarters of a century later, and the proof of the proposition now seems overwhelming.

If Darwin had known the cause of seismic sea waves, and had seen how trenches are dug out in the sea bottom by the expulsion of lava from beneath the sea under the land, can anyone doubt that he would have discovered and proved the leakage of the oceans, and developed the correct theory of mountain formation?

§ 55. *On the Oscillatory Movements of the Crust Shown in the Coal Measures.*—In view of the results established in this paper we need not dwell on the coal measures, and other evidences of the oscillation of the earth's crust. It suffices to say that these oscillations actually took place, as geologists have long believed. The coal fields in Pennsylvania were formed by vegetation growing rapidly and with great luxuriance over areas near the sea level which were again and again elevated and as often depressed by earthquakes. When the land was under the sea the vegetation died out, and mud and shale were deposited; when the area was again upraised another layer of vegetation was produced, and sometimes it was deposited by floods, currents, and drifting where it had not grown. This was during the Carboniferous Age, and while all the land was near the level of the ocean.

The details of such inquiries must be left to geologists and paleontologists, who study the flora and fauna of past ages. Our aim in these papers has been to give a firm basis for legitimate study and speculation, without which the phenomena of nature remain unintelligible. The progress of the sciences of the earth requires two conditions: first, true physical causes; and second, the intelligent and consistent application of these causes to the explanation

of the phenomena, both of the animate and inanimate world. The physicist must content himself with showing the mechanical causes at work and their mode of operation, while the geologist and paleontologist may deal with the evidences of life under these known conditions.

§ 56. *The Equilibrium of the Earth between the Land and Water Hemispheres Explained by the Intumescence of the Land Arising from the Expulsion of Porous Lava from under the Bed of the Sea.* —The remarkable equilibrium preserved by the earth between the land and water hemispheres has long been a matter of speculation among philosophers. Sir John Herschel justly remarked that the high altitude of the continents in the land hemisphere would be most easily accounted for by an intumescence of the land. Pratt has since treated the question in a convincing manner, and shown that the solid parts of the earth's crust beneath the water hemisphere, with pole in New Zealand, must be denser than in the corresponding parts on the opposite side, otherwise the water would flow away towards the land hemisphere and tend to submerge it more completely. (Cf. "Figure of the Earth," 3d edition, pp. 159–160.) Hence he concludes that

"There must therefore be some excess of matter in the solid parts of the earth between the Pacific ocean, and the earth's center which retains the water in its place."

When Pratt wrote this forty years ago there was no suspicion of an intumescent layer beneath the land due to the expulsion of porous lava from beneath the bed of the sea, and accordingly he added that

"This effect may be produced in an infinite variety of ways; and therefore, without data, it is useless to speculate regarding the arrangement of matter which actually exists in the solid parts below."

Now, however, it is proved that the plateaus and continents have been uplifted by intumescent matter expelled from under the sea; and consequently we have data for speculating on how the observed effect is produced.

It is clear that all the great plateaus of the globe and even the continents themselves are underlaid by material lighter than the average of the earth's crust. Naturally the effects are greatest

where the plateaus are highest, as in Himalayas and Tibet, where the deficiency in the attraction of these elevated masses long ago attracted attention. In his "Account of the Operations of the Great Trigonometric Survey of India," Calcutta, 1879, General J. T. Walker says:

"There appears to be no escape form the conclusion that there is a more or less marked negative variation of gravity over the whole of the Indian continent, and that the magnitude of this variation is somehow connected with the height.

"Pratt's calculations had reference only to the visible mountain and oceanic masses and their attractive influences—the former positive, the latter negative—in a horizontal direction; he had no data for investigating the density of the crust of the earth below either the mountains on the one hand, or the bed of ocean on the other. The pendulum observations furnished the first direct measures of the vertical forces of gravity in different localities which were obtained, and these measures revealed two broad facts regarding the disposition of the invisible matter below; first, that the force of gravity diminishes as the mountains are approached, and is very much less on the summit of the highly elevated Himalayan table-lands than can be accounted for otherwise than by a deficiency of matter below; secondly, that it increases as the ocean is approached, and is greater on islands than can be accounted for otherwise than by an excess of matter below. Assuming gravity to be normal (in amount) on coast lines, the mean observed increase at the islands stations was such as to cause a seconds' pendulum to gain three seconds daily, and the mean observed decrease in the interior of the continent would have caused the pendulum to lose 2½ seconds daily at stations averaging 1,200 feet above the sea level, 5 seconds at 3,800 feet, and about 22 seconds at 15,400 feet—the highest elevation reached— in excess of the normal loss of rate due to the height above the sea."

The facts here mentioned by General Walker are recognized in geodesy as applying in different degrees to all the elevated table-lands and mountainous regions of the globe. The physical cause of this deficiency in attraction is now established beyond all doubt, and the intumescence of the land, first suggested by Sir John Herschel, is shown to have arisen from the expulsion of lava from beneath the sea. Thus arises the physical condition which secures the equilibrium of the earth between the land and water hemispheres. This must be regarded as not the least remarkable among several interesting results on the physics of the earth deduced from the principle of the secular leakage of the oceans. Earthquakes, volcanoes, mountain formation, the uplift of islands, plat-

FIG. 15. Map Showing the World Ridge. (From Frye's Complete Geography, by permission of Ginn & Co., Publishers.) It will be noticed that the high mountains and great plateaus everywhere face the outside, which is towards the water hemisphere. This map therefore bears impressive testimony to the truth of the New Theory, and the World Ridge stands as an everlasting witness to the secular action of the oceans in uplifting the land hemisphere of the globe.

eaus, and continents, seismic sea waves, trenches and holes in the bottom of the sea, the feeble attraction of mountains, and plateaus, the equilibrium of the globe between the land and water hemispheres, are all closely related and dependent upon a single physical cause.

FIG. 16. Water Hemisphere, which has the World Ridge around it, drawn by W. R. Smith, of Mare Island.

In view of the order and harmony thus established among these varied phenomena, who will not concur in the view of the great Newton that "Nature is pleased with simplicity and affects not the pomp of superfluous causes"?

Conclusions.

Some of the chief conclusions reached in this and the preceding papers on the physics of the earth may be briefly summarized as follows:

1. The theory of the secular leakage of the oceans explains satisfactorily six great classes of phenomena, not heretofore closely associated, namely: (1) Earthquakes, (2) volcanoes, (3) mountain formation, (4) the formation of islands, plateaus and continents, (5) seismic sea waves, (6) the feeble attraction of mountains and plateaus long noticed in geodesy.

2. And the theory not only explains the leading facts of each class of phenomena separately, but also in relation to all the other classes of phenomena; and this harmonious mutual relationship of all the phenomena proves the theory to rest on a true physical cause.

3. A *vera causa,* once established, should not only explain all the phenomena, and all the relations, but also exclude the consideration of other possible causes, by necessary and sufficient conditions. This alone ensures the entire validity of the reasoning, and the present theory meets this severe test perfectly.

4. We have traced the details of the processes involved in mountain formation, and have exhibited illustrations of its working by processes now observed in the depths of the sea. All stages of mountain formation are thus brought out, and they are all shown to be consistent with this simple theory, which explains the principal phenomena of the earth's crust.

5. This theory explains the distribution of mountains about the continents, their great height which the contraction theory cannot account for; the formation of parallel ridges by the uplift of the side of the trough nearest the sea, when the bottom has so far subsided that the folding up of the nearer side becomes the path of least resistance in the expulsion of molten rocks from under the sea.

6. Several successive troughs are often thus dug out, with ridges forced up between them; and when the whole is raised above the water we have a series of parallel ranges, such as the Allegheny, Tuscarora and Blue Ridge Mountains in Pennsylvania and Vir-

ginia. Heretofore these vast billows of the earth's crust have been utterly bewildering to the naturalist.

7. When several such trenches have been dug out, and the expulsion of lava is from both sides, as happens when the sea is thus distributed, the ridges may finally be forced up and so crowded together from both sides that overturned dips and inverted strata are produced, as in the Swiss Alps. No previous theory has been adequate to account for this amazing phenomenon, the explanation of which is thus seen to be exceedingly simple. This test may be justly considered the *experimentum crucis* of the theories of mountain formation.

8. The Andes in South America are nothing but a vast wall or embankment erected by the Pacific Ocean, through the expulsion of lava, along its border. Hence the persistence of the earthquake belt and seismic sea waves along this coast.

9. This embankment includes not only the peaks and chains of mountains, large and small, in the Eastern and Western Cordillera, but also the intervening plateaus, such as those of Quito, Caxamarca, Cuzco, and Titicaca.

10. The molten rock expelled from under the sea is lighter than average material of the layer below the earth's crust, and when the included vapor of steam is allowed to expand, as in volcanoes, pumice is formed, and often blow out in vast quantities. Pumice of various degrees of density underlies the mountain chains, and some of it is blown out of those mountains which become volcanoes.

11. The way in which these plateaus are interwoven with the Andes mountains shows that the whole embankment is due to the continued action of one common cause. And since the mountains were uplifted by the expulsion of lava from under the sea, as proved by the uplifting of the land in earthquakes and the sinking of the sea bottom, indicated by the accompanying seismic sea waves, it follows that the plateaus also are underlaid by matter lighter than the average, which has been expelled from under the ocean.

12. The total quantity of matter thus expelled from beneath the ocean is very large, but it is the result of an infinite number of earthquakes and seismic sea waves during past geological ages. This circumstance affords us an idea of the immense age of the

Andes Mountains, which are the youngest of the great mountain systems of the globe.

13. The terrible fracturing of the crust in the sharp folding involved in the formation of the Andes enabled a vast number of volcanoes to break out, and about one hundred and five have been active within historical times.

14. The formation and activity of the volcanoes in the Aleutian and Japanese Islands is similar to those in the Andes, and represent conditions suitable to the maximum development of volcanic activity. These are sharp folds of the crust near a deep sea from which the expulsion of lava is rapid and violent.

15. The connection of earthquakes with volcanoes and of both phenomena with the sea is clearly established by the geographical distribution and by the vapor of steam emitted by volcanoes. The nature of the underlying material is shown by the ashes, cinders, pumice and lava forced out by the accumulating subterranean steam pressure.

16. Earthquakes, however, are the more general, volcanoes the more special phenomena. The mountains are formed by the sea, but only a few of the peaks break out into volcanoes. No volcano long remains active very far from the ocean or other large body of water, because as the lava hardens in the throat of the volcano the supply of steam is inadequate to maintain activity.

17. If we consider the innumerable islands in the sea, it is evident that they too have been uplifted by earthquakes. Sometimes the sea bottom near them has been undermined in the process of uplifting, and afterwards sunk down, making an adjacent hole in the bottom, and producing seismic sea waves of the first class, as in mountain formation where trenches are being dug out near the continents.

18. Seismic sea waves of the second class are produced by the uplift of the sea bottom, into ridges, or submarine plateaus and islands. In such cases the water rises suddenly without previously withdrawing from the shore.

19. But seismic sea waves of the first class due to the sinking of the sea bottom, after it is undermined by the expulsion of lava, are the most important and most celebrated. The waves at Helike,

373 B. C.; Callao, 1746; Lisbon, 1755; Arica, 1868; Iquique, 1877; Japan, 1896, were all of this class.

20. We may pass directly from the Andes to the Himalayas, and from the high plateaus of South America to those of Asia. Just as the plateaus from Quito to Titicaca were formed by the expulsion of matter from under the Pacific, so also those of Tibet and Iran are due mainly to the expulsion of lava from beneath the Indian and Pacific Oceans.

21. In the case of the plateau of Tibet the resulting uplift is partly due to the combined action of the Pacific, which thus folded the ranges to the East. With two oceans so large and deep as the Indian and Pacific coöperating in this uplift, it is no wonder that the maximum effect was produced and that Tibet became the highest plateau in the world.

22. The Himalayas are higher and further from the sea than the Andes, but the earthquake belt at the base still persists in both cases, and the configuration in regard to the sea shows that the causes at work to produce these mighty uplifts were absolutely similar. And if the mountains are due to the same cause, the plateaus are also.

23. The total height of Tibet is only about one sixth or seventh of the thickness of the earth's crust, and hence the uplift, great as it is, is not such as would necessarily produce great volcanic outbreaks at the surface.

24. Great lava flows, however, occurred in India, and some volcanic phenomenon are known in the Himalayas, but our knowledge of these mountains is not yet adequate to enable one to estimate just how much volcanic activity developed there.

25. Great lava flows are due to the rupture of the crust, by the opening of a fault near the sea, not to volcanic outbreaks. These flows are seen in Utah, Oregon and India, on a scale commensurate with the forces which have uplifted the mountains and plateaus.

26. One may pass directly from the mountains and plateaus of South America to those of Asia, and then to those on the Pacific slope of North America, by the most gradual stages.

27. In this transition the processes are so similar and the differences so small, that it is impossible to deny that the mountains

and plateaus west of the Rocky Mountains were all formed through the uplift of the land by the Pacific Ocean.

28. The North American Plateau is larger, but correspondingly lower than those in Asia, so that the volume of material involved in the two uplifts is comparable. Thus all the great plateaus of the globe are due to the action of the sea, in the course of immeasurable ages. The slowness of the process conveys the best conception of the vast interval of time since the consolidation of the globe.

29. Charles Darwin long ago held that "the forces which slowly and by little starts uplift continents and those which at successive periods pour forth volcanic matter from open orifices are identical." He showed that the southern end of South America has recently risen from the sea, and Professor Suess has shown that the universal lowering of the strand line throughout the principal countries gives a similar indication for all the lands of the globe.

30. If one end of a continent can be raised by earthquake forces depending on the sea, then obviously a whole continent can be raised by these forces; and similar uplifts can occur for all the continents in both hemispheres. The vast vertical walls of granite so often found rising from the sea in South America and elsewhere have clearly been uplifted by earthquakes.

31. We therefore reach the conclusion that the forces which have raised the mountains, islands and plateaus, have also raised the continents and established the equilibrium of the globe between the land and water hemispheres. This force is nothing else than common steam, operating through the expansion of molten rock beneath the crust and arises principally from the secular leakage of the ocean bottoms.

32. The main effect of earthquakes is the production of more land. The continents are being lifted out of the sea, in spite of erosion, as we see by the withdrawal of the oceans to a greater and greater distance from old mountain chains, such as the Rocky Mountains and Appalachians in America and the Alps in Europe.

33. But for this uplift of the land by the leakage of the oceans none of the higher forms of life could have developed upon the earth. The climate and drainage of all continents have been largely

determined by these forces, which have produced the mountains and river systems of the world.

34. We cannot prove by experiments on rock twenty miles thick that it will leak under the pressure of the ocean, but we can observe the surface movements in earthquakes such as occur in Alaska, where lava is being expelled from under the ocean and pushed under the land.

35. This movement is everywhere in the same direction, whether in Alaska, Japan, the Antandes, South America, or elsewhere—namely from the ocean towards the land. The reason of this is that much steam is formed under the oceans, but scarcely any under the land, and hence it pushes up the crust along the edge of the continents and finally almost walls them in with mountains, as was long ago pointed out by Dana.

36. The old theory of secular cooling and contraction of the globe is false and misleading, and all who have carefully examined it agree that it is totally inadequate to account for terrestrial phenomena. In fact so far from contracting it seems certain that the earth is actually undergoing a slow secular expansion.

37. The Rev. O. Fisher and Major Dutton were among the earliest to reject this theory as incapable of explaining mountain ranges. But it is remarkable that after the contraction theory was proved to be unsatisfactory, it continued to be used in all works on geology and kindred sciences, and indeed still is accepted by those who adhere to the antiquated doctrine that the earth is a failing structure. Such views had some justification a quarter of a century ago; today they are absolutely without excuse.

38. There are the best grounds for accepting the doctrine of Isostacy, as approximately true for the earth at all times; consequently there are no sensible stress-differences, or tendencies to flow, except in the layers just beneath the crust. At greater depths the matter of the earth is made solid by pressure, being at the centre about three times more rigid than nickel steel. Hence deep down the earth is now and always has been quiescent. The only layer of the earth which is plastic and perhaps viscous is that just beneath the crust; this layer flows under the tremendous forces at work in earthquake movements. It is the movement of this molten rock be-

neath the crust, chiefly when it is expelled from under the sea, which shakes down cities and devastates whole countries.

39. This expulsion of lava under the land can mean nothing else than the secular leakage of the oceans, because the mountains along the coast which are rent by the shaking of the earth till they break into volcanoes, emit chiefly vapor of steam. Moreover the unsymmetrical shape of the mountain folds, showing the gentler slope towards the shore, indicates that the folds of the crust were pushed from the direction of the sea. This was produced by the expulsion of lava under the crust arising from the secular leakage of the ocean bottom.

40. By the study of seismic and other phenomena now observed in the great laboratory of nature we may penetrate the deepest secrets hidden beneath the earth's crust, to which no mortal eye can ever bear direct witness. And these researches may greatly increase the safety of whole communities, and especially of cities and of commerce, throughout the world, by enabling us to guard against the dangers of earthquakes and seismic sea waves. This appropriate use of the laboratory of Nature is one of the ultimate objects of natural philosophy.

Blue Ridge on Loutre,
Montgomery City, Missouri,
February 19, 1908.

POSTSCRIPT.

In a paper read before the Royal Society in 1902, Professor J. H. Jeans, formerly of Cambridge, now of Princeton University, has the following theory of earthquakes:

"It seems to be almost certain that the present elastic constants of the earth are such that a state of symmetrical symmetry would be one of stable equilibrium. On the other hand, if we look backward through the history of our planet, we probably come to a time when the rigidity was so much that the stable configuration of equilibriums would be unsymmetrical. At this time the earth would be pear-shaped and the transition to the present approximately spherical form would take place through a series of ruptures. It is suggested that the earth, in spite of this series of ruptures, still retains traces of a pear-shaped configuration. Such a configuration should possess a single axis of symmetry, and this, it is suggested, is an axis which meets the earth's surface somewhere in the neighbourhood of England (or possibly some hundreds of miles to the southwest of England). Starting from

England we find that England is at the centre of a hemisphere which is practically all land; this would be the blunt end of our pear. Bounding the hemisphere we have a great circle, of which England is the pole, and it is over this circle that earthquakes and volcanoes are of most frequent occurrence. Now, if we suppose our pear contracting to a spherical shape, we notice that it would probably be in the neighbourhood of its equator that the changes in curvature and the relative displacements would be greatest, and hence we would expect to find earthquakes and volcanoes in greatest number near this circle. Passing still further from England, we come to a great region of deep seas, the Pacific, South Atlantic, and Indian Oceans; these may mark the place where the 'waist' of the pear occurred. Lastly we come almost to the antipodes of England, to the Australian continent. This may mark the remains of the stalk-end of the pear." (*Nature*, Vol. LXVII., p. 190.)

After what has been shown in this series of papers, it is unnecessary to dwell upon this hypothesis of Professor Jeans, which has the merit of originality; but we may remark that if it gave a true view of the physics of the earth, there should be a belt around the globe of at least the width of the terrestrial radius, over which the earthquakes are about equally distributed, whereas in fact they are felt principally along the margins of the Pacific Ocean. The observed earthquake belt on land is so narrow that it is clearly impossible to ascribe the effects to this supposed adjustment of the earth's figure. And of course it fails totally to account for the sinking of the sea bottom and the uplift of the coast, which is typical of mountain formation.

THE ABSORPTION SPECTRA OF NEODYMIUM CHLORIDE AND PRASEODYMIUM CHLORIDE IN WATER, METHYL ALCOHOL, ETHYL ALCOHOL AND MIXTURES OF THESE SOLVENTS.

(With six plates.)

TWENTY-FIRST COMMUNICATION.

BY HARRY C. JONES AND JOHN A. ANDERSON.

(*Read April 25, 1908.*)

(This is a preliminary report on part of an investigation carried out with the aid of a Grant from the Carnegie Institution of Washington.)

The absorption spectra of salts of cobalt, nickel, copper, iron, chromium, neodymium, praseodymium and erbium have been studied in the present investigation. Of these the salts of neodymium and praseodymium are perhaps the most interesting and important. This is due to the large number of absorption bands shown by these substances, and, further, to the very unusually sharp character of these bands.

The method employed in making the spectrograms consists in allowing light from a spark, or from a Nernst filament, to pass through the solution in question, fall upon a grating and then upon the photographic plate.

For visual work a small direct vision grating pocket spectroscope was found very convenient and useful. For photographing the spectra the vertical grating spectroscope used by Jones and Uhler[1] was employed.

In making the photographs the Seed L-ortho film was used for the region from λ 2000 to about λ 6000.

For photographing the red end of the spectrum a Wratten and Wainwright panchromatic glass plate was used.

[1] Carnegie Publication No. 60.

The Nernst filament was found to be the most satisfactory source of light from the extreme red to the beginning of the ultra-violet. It is sufficiently brilliant to require an exposure of only a minute, but practically ceases at about λ 3200. For wave-lengths shorter than this some spark spectrum must be used.

The cadmium-zinc spark used by Jones and Uhler was fairly satisfactory, especially in the extreme ultra-violet, but has the drawback that there are present a limited number of very intense lines, on a rather faint continuous background. We tried to obtain a spark spectrum having a very large number of lines, but with no lines of very great intensity. We found that tungsten, molybdenum and uranium all satisfied these requirements.

The terminals finally used were prepared by dipping pieces of carbon in a concentrated solution of ammonium molybdate, and then heating in a bunsen burner. They were then dipped into a solution of uranium nitrate and similarly heated.

The coil used to produce the spark was a large Röntgen X-ray coil.

Making a Spectrogram.

In making a spectrogram consisting of seven photographic strips, the following mode of procedure was adopted: Seven separate solutions were made up of the desired strengths. The cell[2] to be used was filled to the required depth with the most concentrated solution of the series, and the quartz plates determining the depth of the solution adjusted to parallelism. The exposure to the Nernst lamp was then made, being usually one minute long. An opaque screen covering up the visible spectrum as far down as λ 4000 was then interposed between the grating and the photographic film, and the exposure to the light of the spark in the ultra-violet made. The duration of this exposure was usually about two minutes. The photographic film was then moved into the proper position for the next exposure. The above series of operations was then repeated for each of the succeeding strips.

After the film had been exposed for each solution and the spark spectrum impressed, it was necessary to make a similar series of

[2] See Carnegie Publication No. 60.

exposures on a panchromatic plate for the red end of the spectrum, using the same set of solutions.

The scale accompanying the spectrograms was made by photographing an ordinary paper scale. Several photographs were taken, the distance between the paper scale and the lens of the camera being varied slightly from exposure to exposure. The resulting negative which fitted the majority of spectrograms best was selected and used throughout.

Neodymium Chloride in Water—Beer's Law.

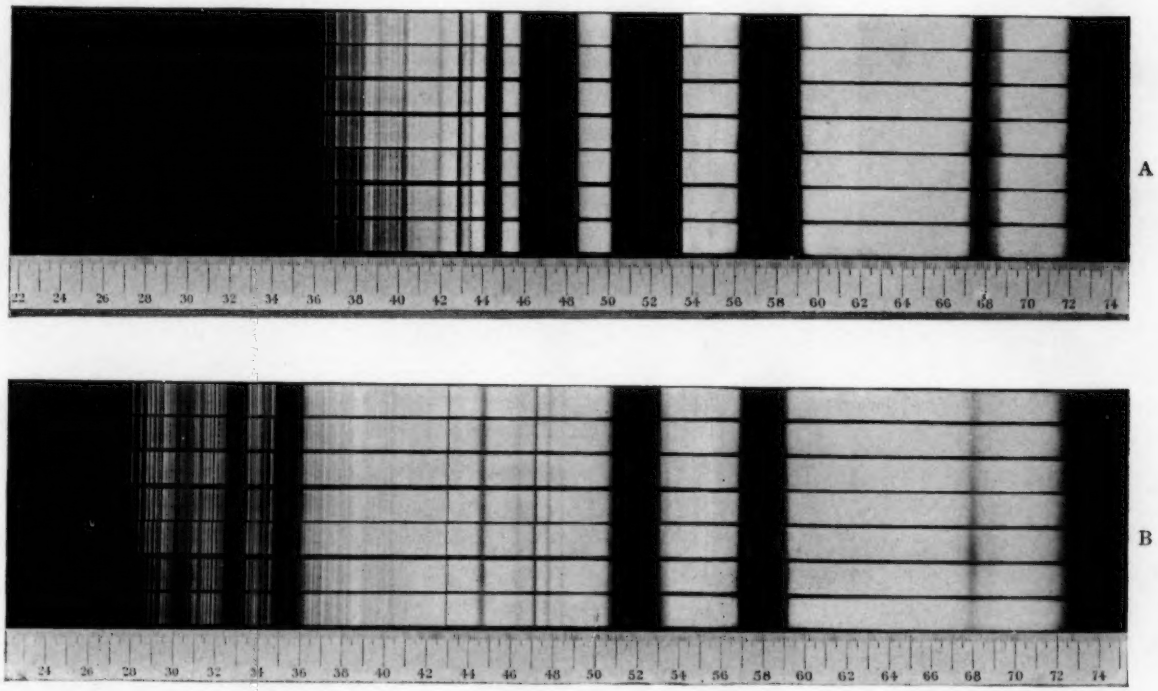
(See plate 1.)

The concentrations of the solutions of neodymium chloride were so chosen and the depths of cell so selected that the total amount of coloring matter in the path of the beam of light was kept constant. From Beer's Law the absorption shown by the several solutions, under these conditions, should be the same. The concentrations of the solutions used in making the negative for *a*, plate 1, beginning with the one whose spectrum is adjacent to the numbered scale, were 3.40, 3.02, 2.72, 2.38, 2.17, 1.90 and 1.70; the corresponding depths of cell being 12, 13.5, 15, 17, 19, 21.5 and 24 mm. For *b*, plate 1, the concentrations were 3.40, 2.55, 1.70, 1.13, 0.80, 0.57 and 0.43; the corresponding depths of absorbing layer being 3, 4, 6, 9, 13, 18 and 24 mm.

The most concentrated solutions appeared brownish yellow in their bottles, from which the color changed on dilution to a yellowish pink, the color being extremely faint in the most dilute solutions.

The exposures to the light of the Nernst lamp and spark were, respectively, 1 minute and 2 minutes; the slit having a width of 0.01 cm. The exposures and slit width were not varied in the work recorded in the present chapter, the object being to make the spectrograms as nearly comparable as possible.

Both *a* and *b* of plate 1 show the presence of some general absorption in the ultra-violet, which decreases quite rapidly with dilution. The absorption bands also narrow somewhat with decrease in concentration, especially from 3.4 normal to about 1.7 normal. For concentrations less than about 1.5 normal Beer's Law seems to hold very accurately indeed, with the exception of the


A
B

shading towards the red accompanying the band near λ 5800, which seems to decrease somewhat with dilution for concentrations of one normal or less.

In the following table the measurements of the positions of the bands were made on the seventh strips of *a*, plate 1, and, therefore, refer to a concentration of 1.7 normal with a depth of layer of 24 mm. The remarks referring to changes with dilution apply to a change in concentration from 3.4 to 1.7 normal, the depths of layer being so varied that the product of concentration and depth remains constant.

λ	Character.	Remarks
2810	Faint transmission begins.	
2890–2910	Band with well defined sharp edges.	
2970–2995	A double band, strongest component to violet.	The observed narrowing with dilution perhaps due largely to general U. V. absorption.
3220–3330	Strong band of complete absorption, sharp edges.	Narrows slightly with dilution.
3380–3400	Rather faint band, most intense towards red.	
3435–3595	Complete absorption, edges sharp.	
4180	Hazy, not very intense.	Narrows some with dilution.
4275	Very intense and sharp.	Narrows considerably at first.
4290	Narrow and faint.	Between this and λ 4275 is fairly strong absorption in the most concentrated solution. This absorption has disappeared in the spectrum measured.
4330	Hazy.	
4410–4465	Edges rather hazy.	This band is coincident with band due to praseodymium, and is to be ascribed to this element which has not been completely separated from the neodymium. It does not change with dilution.
4580–4650	Band with hazy edges not completely separated from λ 4665–λ 4710.	Narrows slightly with dilution.
4665–4710	More sharply defined on red than on violet side.	Partly due to praseodymium. Does not change with dilution.

λ	Character.	Remarks.
4740–4770	Fairly sharp edges.	Not affected by dilution.
4820	Hazy on violet side.	Due at least partly to praseodymium.
5000–5330	Red limit sharp, violet a little hazy.	Violet shading a little greater in concentrated solutions.
5660–5930	Violet limit sharp. Red edge hazy.	Shading on red side decreases with dilution.
6235	First and strongest band in orange group.	Not affected by dilution.
6260	Narrow and rather faint.	Not affected by dilution.
6270–6310	Faint band.	Not affected by dilution.
6360–6390	Faint band.	Not affected by dilution.
6730	Faint, in shading of principal red band.	Not affected by dilution.
6770–6840	Principal red band. Edges hazy.	Not affected by dilution.
6890	Band with hazy edges.	Not affected by dilution.
7250	End of transmission.	Not affected by dilution.

The most marked change produced by dilution from 3.4 to 1.7 normal, excepting that in the red shading of the λ 5660–5930 band, is that taking place on the red side of the narrow absorption line at λ 4275. In the spectrum of the most concentrated solution the red edge of this line falls at λ 4280, from which place a uniform absorption extends to λ 4295. In the third spectrum, counting from the numbered scale, the shading has almost completely disappeared, leaving a very narrow line at approximately λ 4290. The width of this line is only 2 or 3 A. U. and it persists with unchanged intensity throughout the remaining strips of the spectrogram. Its intensity is, however, not sufficient to make it show in the reproduction, and not even great enough to make it visible on the negative for *b*, plate 1.

The limits of transmission for the yellow band, as shown by the spectrum of the most concentrated solution, are λ 5660 and λ 5950; hence the narrowing of its red side amounts to 20 A. U.

b, plate 1, starts at the same concentration as *a*, but the effective depth of absorbing layer is only one-fourth of that used in *a*. Hence this spectrogram represents the spectrum of a solution of neodymium chloride 24 mm. deep and having a concentration of 0.43 normal. The absorption bands are all much narrower, and

several of them are shown in the process of breaking up into simpler bands. The bands in the ultra-violet have disappeared excepting the one at λ 3435–λ 3595, which is still intense, and a trace of the one at λ 3220–λ 3330. Transmission in this region now extends faintly to λ 2460. No new absorption bands beyond λ 2800 can be seen.

The λ 3435–λ 3595 band now has the limits λ 3450–λ 3580, and shows a weak transmission at λ 3485, which increases somewhat with dilution, thus dividing the band into two.

The band at λ 4180 is weak throughout *b*, plate 1.

The band having its middle at λ 4445, perhaps due entirely to praseodymium, in *a*, plate 1, has about the same intensity as it shows in a solution of praseodymium chloride having a concentration of 0.85 and a depth of absorbing layer equal to 3 mm. This indicates that the percentage of praseodymium in the neodymium salts used was about 6 per cent. The band at λ 4825 partly due to praseodymium may also be seen throughout the entire series under consideration. The wave-length of the praseodymium band being λ 4815, while that of the band showing in all the neodymium spectra has the position λ 4825, showing that neodymium has a band nearly coincident with that given by praseodymium, but lying a little closer to the red end of the spectrum. The remaining praseodymium band has the position λ 4685, this nearly coinciding with the rather narrow, strong neodymium band whose position is λ 4695.

The band which under *a*, plate 1, was recorded as having the limits λ 4580–λ 4650, shows in *b* as a hazy band with its center at 4615, together with a narrow faint line at λ 4645.

The band which in the table is recorded as λ 4740–λ 4770 has in *b*, plate 1, become a slightly hazy band having its middle at λ 4760. Its intensity is intermediate between that of the bands at λ 4695 and λ 4825.

The band which in *a*, plate 1, has the limits λ 5000–λ 5330, breaks up into a rather complicated series of bands on dilution, some idea of which may perhaps be gained from the following: *b*, plate 1, shows some absorption throughout the region given, but with a deep, narrow band at λ 5090, and faint transmission at λ 5100 and in the region λ 5150–λ 5180. Absorption is complete from λ 5105

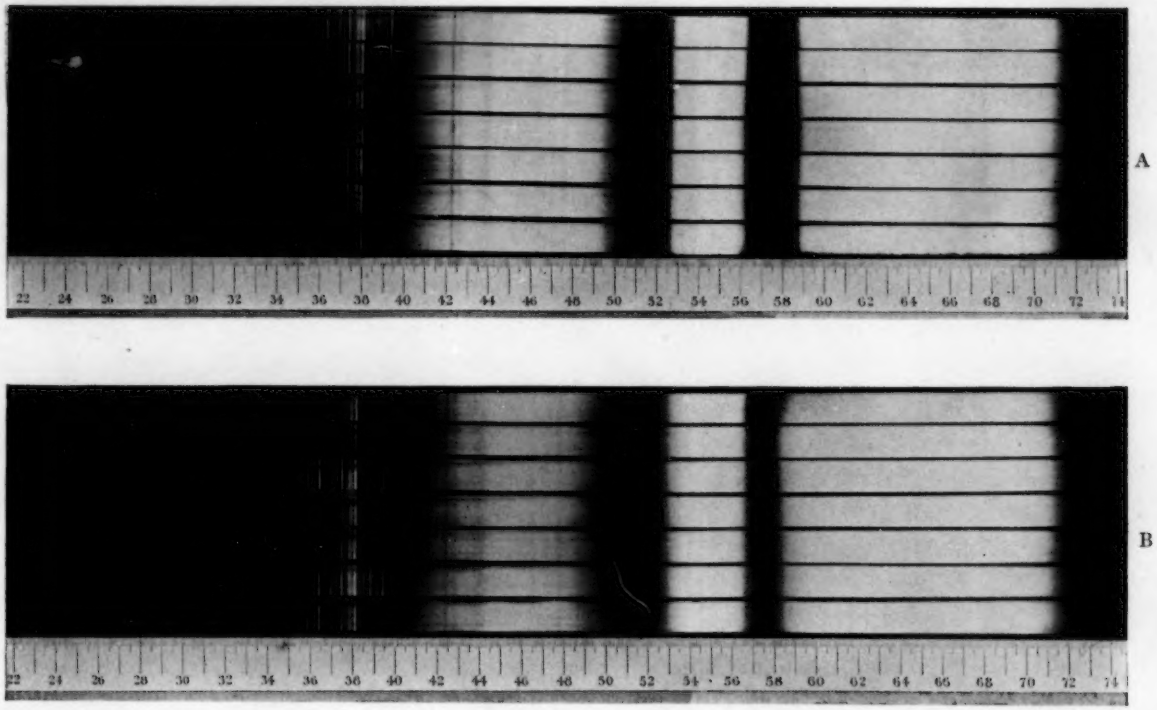
to λ 5150, and from λ 5180 to λ 5270. There is again incomplete absorption from λ 5270 to λ 5330, with indication of a band at λ 5315.

The limits of the yellow band in *b*, plate 1, are λ 5700–λ 5880, in the strip corresponding to the most concentrated solution. The band narrows by 30 Angström units on this spectrogram, the narrowing being due to a decrease in the shading towards the red, with decrease in concentration.

The most intense bands of neodymium chloride, and hence the ones which would be most conspicuous in a very dilute solution are the following: λ 3465, λ 3540, λ 4275, λ 5205, λ 5225, λ 5745, λ 5765 and λ 7325.

The wave-lengths of all the bands are collected in the following table, together with a brief description of the appearance of each band. It is to be understood that this table is not meant to represent what could be seen or photographed in any one solution of neodymium chloride in water. It merely records the positions of all the bands that can be seen in a layer from 3 to 12 mm. deep, when the concentration is varied from 0 to 3.4 normal.

λ	Description.
2900	About 20 A. U. wide.
2985	About 25 A. U. wide.
3225	Narrow and sharp.
3390	Narrow, faint.
3465	Very intense, narrow.
3505	Rather wide.
3540	Very intense, narrow.
3560	Faint, narrow.
4180	Faint, hazy.
4275	Very intense and sharp.
4290	Very narrow, faint.
4330	Hazy edges.
4615	Rather wide and hazy.
4645	Very narrow, faint.
4695	Narrow, intense.
4760	Hazy edges, fairly narrow.
4825	Narrow and fairly intense.
5090	Narrow, intense.
5125	Rather wide and hazy.
5205	Very intense, narrow.
5222	Very intense, narrow.


A
B
22 24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74

5255	Narrow, intense.
5315	Hazy edges, faint.
5725	Narrow, intense.
5745	Very intense.
5765	Very intense.
5795	Intense, moderately narrow.
5830	Very faint and hazy.
6235	Fairly narrow.
6260	Very narrow, faint.
6270–6310	Faint, hazy edges.
6360–6390	Faint, hazy edges.
6730	Faint band.
6800	Moderately intense, hazy edges.
6890	Hazy edges.
7325	Very intense and narrow.
7350	Narrow.
7390	Rather wide band.

Neodymium Chloride in Methyl Alcohol—Beer's Law.

(See plate 2.)

The concentrations of the solutions used in making the negative for *a*, beginning with the one whose spectrum is adjacent to the numbered scale were 0.50, 0.40, 0.315, 0.25, 0.20, 0.16 and 0.125; the corresponding depths of absorbing layer being 6, 7.5, 9.5, 12, 15, 19 and 24 mm. The concentrations for *b* were in the same order 0.20, 0.16, 0.13, 0.10, 0.08, 0.06 and 0.05, the depths of cell being the same as used in *a*.

There is some absorption in the extreme ultra-violet, which is to be ascribed to the solvent, however, and not to the neodymium chloride.

No trace of absorption due to the dissolved substance is visible until we reach the group of bands near λ 3500. These are three bands having their centers at λ 3475, λ 3505, and λ 3560. Of these the one at λ 3560 is the widest and also the most intense; the one at λ 3475 being somewhat fainter than that at λ 3505. The bands are all much wider and hazier than those occurring near the same place in the aqueous solution. No change with dilution, indicating a deviation from Beer's Law, can be detected in these or any of the other bands in the alcoholic solutions of the chloride.

In the violet and blue regions we find the following band at

λ 4290, about 10 A. U. wide and only moderately intense. At λ 4325 a band somewhat wider and fainter. At λ 4460, a rather wide hazy band with a faint hazy companion towards the violet. This is the band which is perhaps due to praseodymium. The much greater concentration of the alcoholic solutions of praseodymium chloride studied in this work, makes it impossible to verify this by seeing whether the praseodymium band in dilute solution really has this general character.

There are bands at λ 4700, λ 4780 and λ 4825, all of about the same intensity; the one at λ 4770 being, however, much narrower than the other two, of which λ 4825 is somewhat the wider. Both λ 4700 and λ 4780 have faint companions to the violet.

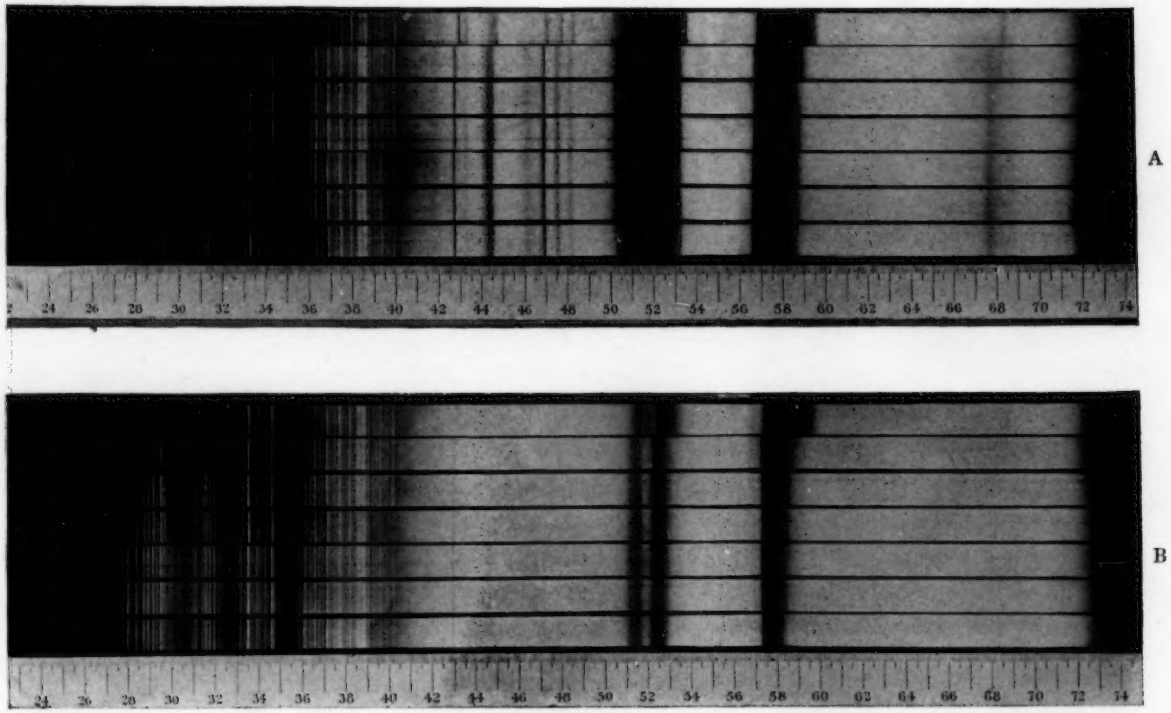
The group in the green is made up of six bands as follows: λ 5125 hazy and rather wide, moderately intense; λ 5180, also hazy but much fainter; λ 5220 moderately intense and narrow; λ 5245 intense and with faint companion towards the red; λ 5290 narrow and moderately intense. Shading as far as λ 5330 with indications of faint band at λ 5315.

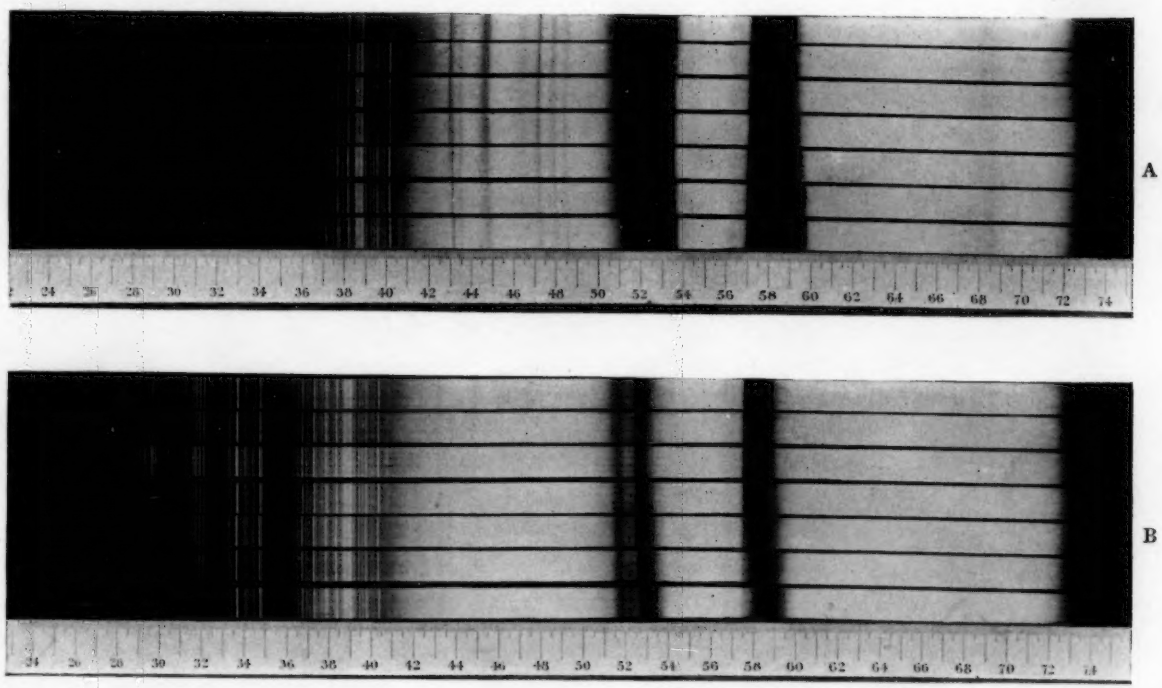
The yellow group is made up of seven bands having the following characteristics: λ 5725 moderately intense with hazy edges; λ 5765 narrower, but not quite as intense as λ 5725; λ 5800 fairly narrow, strong; λ 5835 very intense; λ 5860 hazy and moderately intense; not clearly separated from λ 5835 shading to λ 5970, with two faint bands superposed on it, one at λ 5895 and the other at λ 5925.

No trace of bands is to be seen in the orange, but in the red there is a fairly narrow but faint band at λ 6860. The spectrum ends at λ 7355 in a deep, rather narrow band. It is evident that the spectrum of neodymium chloride when dissolved in methyl alcohol is quite different from its spectrum in aqueous solution.

Neodymium Chloride in Mixtures of Methyl Alcohol and Water. (See plates 3 and 4.)

Since, as we have just seen, the absorption spectrum of neodymium chloride in aqueous solution is so different from that of the alcoholic solutions, it was thought to be of some interest to see how the change from one to the other would take place if one of the


A
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74
B
24 26 28 30 32 34 36 38 40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74


A
B

solvents was made to displace the other gradually. A series of solutions was accordingly made up, the concentration of the dissolved salt being constant and equal to 0.5 normal, but the character of the solvent varying as follows: The percentages of water in the seven solutions were 0, $16\frac{2}{3}$, $33\frac{1}{3}$, 50, $66\frac{2}{3}$, $83\frac{1}{3}$ and 100; the corresponding percentages of methyl alcohol were 100, $83\frac{1}{3}$, $66\frac{2}{3}$, 50, $33\frac{1}{3}$, $16\frac{2}{3}$ and 0. Two spectrograms were made, namely *a*, plate 3, where the depth of the cell was 1.5 cm. and *b* where the cell had a depth of only 5 mm. *a* was made in order to show clearly the change taking place in the narrower and fainter bands, while *b* was intended to show the change of structure of the more intense bands such as the green and yellow ones. The strip which is adjacent to the numbered scale belongs to the solution in pure water, while the one nearest the narrow comparison spark spectrum belongs to the solution in pure methyl alcohol.

Plate 3 shows that beginning with the strip nearest the scale, the first six spectra are very nearly identical. From the sixth to the seventh there is an abrupt change which at first sight consists in a shift of all the bands towards the red, but which on closer examination is seen to consist in a disappearance of one spectrum and the appearance of the other. Since the first strip is the spectrum of the solution in pure water, it follows, since the sixth is nearly identical with the first, that as large a percentage of alcohol in the solvent as 83 per cent. does not change the absorption spectrum materially; the chief change taking place when the percentage of alcohol is varied from 83 per cent. to 100 per cent.

It is to be noted that the apparent shift of the bands towards the red is in reality not quite as great as it appears at first sight from plate 3, owing to the fact that the film accidently shifted slightly towards the red between the sixth and seventh exposures. The amount of this mechanical shift is easily seen, however, by comparing the spark lines in the ultra-violet. A measurement of the shift shows it to be approximately 3 Angström units, and the same for both *a* and *b*, while the "apparent" shift of the absorption line at λ 4275 in aqueous solution is actually 15 Angström units, its position in the alcoholic solution being λ 4290.

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The slight changes taking place with some of the bands throughout the spectrograms of plate 3 are perhaps sufficiently clear in the reproductions. However, as a good deal of the detail shown by the negatives is lost even in the most perfect processes of reproduction, we give here a description of the changes taking place in two of the bands as seen on the original negative. We select the bands at λ 4275, and λ 4760 from the negative for *a*, plate 3.

In the aqueous solution the λ 4275 band is very intense and narrow, its whole width being less than 5 Angström units. The edges are only very slightly shaded. In the alcoholic solution the position of the center of the corresponding band is λ 4290. It has a width of from 12 to 13 Angström units, and is not nearly as intense as in the aqueous solution.

Throughout the first six strips the λ 4275 band maintains its position and intensity almost unchanged. Its position does not change in the least, but its intensity in the sixth strip is a trifle less than in the others. In the seventh strip there is not the faintest trace left of it. In the third strip, corresponding to the solution whose alcohol content was 33⅓ per cent., there appears at λ 4285 an extremely faint and narrow line. In the fourth strip it is somewhat wider and more intense, but its center is still at λ 4285. In the fifth strip it is beginning to be fairly conspicuous, and in the sixth it is a band of moderate intensity having its center at about λ 4287. This band is undoubtedly the same one which in the pure alcoholic solution has its center at λ 4290 or very near there; the exact wavelength being perhaps nearer to λ 4292. We see then that even when the mixed solvent contains only about one-half alcohol this band exists independent of and distinct from the band characteristic of the aqueous solution; that it is at first only a very narrow and faint line which widens towards the red as the percentage of alcohol is increased.

The band whose center is at λ 4760 has the following appearance in the aqueous solution: Faint absorption begins at λ 4748 and rises rapidly to a maximum between λ 4755 and λ 4760, then decreases slowly to nothing at λ 4775. The band is accordingly a trifle asymmetrical, the slope towards the violet being considerably steeper than that towards the red. The corresponding band in the

alcoholic solution is double and answers the following description: Very faint absorption begins at λ 4753 and rises to a faint maximum at about λ 4757, becoming again zero at λ 4760. It begins again at λ 4772, rises rapidly to a strong maximum at λ 4780 and falls to zero at λ 4790. The component whose center is at λ 4757 is very faint compared with the main band.

In the first and second strips we have nothing but the band corresponding to the aqueous solution. In the third strip the red side of the band has increased slightly in intensity, making it appear much more nearly symmetrical. This change increases in the fourth and fifth strips, the band at the same time widening considerably. In the sixth strip its appearance is as follows: Absorption begins at λ 4748 and rises to a maximum just to the violet side of λ 4760, then decreases slightly towards λ 4770, after which it increases somewhat to λ 4778, then falls off to zero at λ 4787.

It is very evident from a study of the change in this band that the two bands characteristic of the aqueous and alcoholic solutions coexist, and that the band appearing in our photographic strips is the sum of the two taken in different proportions. The proportion of the alcohol band being, however, very much smaller than the proportion of alcohol in the corresponding solution. A similar description might be given for any one of the other bands, but this is not necessary as the changes are of exactly the same nature as those we have already indicated. In every case where the alcoholic solution has a strong band, which differs somewhat in position from any band in the aqueous solution, we begin to see traces of this band when the proportion of alcohol in the mixture reaches 50 per cent., but the band remains comparatively faint even when the proportion is as high as $83\frac{1}{3}$ per cent.

In order to study the change which takes place between the sixth and seventh strips of the spectrograms of plate 3, more carefully, a series of alcohol solutions were prepared containing the following percentages of water, 0, $2\frac{2}{3}$, $5\frac{1}{3}$, 8, $10\frac{2}{3}$, $13\frac{1}{3}$ and 16. The concentration of the neodymium chloride was constant and equal to 0.5 normal. Two spectrograms were made, one with a depth of absorbing layer of 1.5 cm., in order to show the fainter bands, and the other with the depth of the cell only 5 mm. in order to show as

much as possible of the structure of the larger bands. The first spectrogram is reproduced as *a*, the second as *b*, plate 4. The strips corresponding to the pure alcohol solutions are adjacent to the numbered scale, the spectrum of the solution containing 16 per cent. water being next to the comparison spark spectrum.

Although we found in considering plate 3 that some slight change in the spectrum takes place where the percentage of alcohol is changed from 0 to 83 per cent., yet this change is so small and the bands due to the aqueous solution are so strong that we may regard the spectrum of a solution containing 16 per cent. of water as practically that of the aqueous solution. Accordingly, the spectrograms on plate 4 may be taken to show very nearly the whole change which takes place when the solvent of neodymium chloride is gradually changed from pure water to pure methyl alcohol.

In *a* the ultra-violet band is rather too intense to allow its structure to be seen. Accordingly, we see the whole band remains sensibly unchanged as the water is varied from 16 per cent. to 8 per cent., and then shifts towards the red with increasing rapidity as the water is reduced to zero; the whole apparent shift amounting to about twenty Angström units. On the negative the intense band at λ 3465 may, however, be clearly seen, and its intensity decreases very slowly from the first to the third strips, counting from the narrow comparison spark spectrum. In the fourth strip its intensity is about half of what it was in the first strip, and from this it decreases rapidly, vanishing entirely in the strip nearest the scale. In *b* the structure of this band is seen very distinctly, and we find that the bands characteristic of the aqueous solution gradually decrease in intensity, especially from the third to the sixth strips, while the wider bands characteristic of the alcoholic solutions increase in intensity, the two sets existing together. The change in the band at λ 4275 is the one that shows the best, because here the two bands belonging to the aqueous and alcoholic solutions, respectively, are both intense and narrow and clearly separated from each other. The alcoholic band is clearly visible in the first strip, and it increases continuously in intensity as the amount of water is decreased, but more rapidly from the fourth to the seventh strips than from the first to the fourth. Its position also shifts somewhat

towards the red from the first to the fourth strips, the wave lengths of its center for the two strips being, respectively, λ 4287 and λ 4292. Accompanying this shift is a change in its character which may be gathered from the following statements: In the first strip it has the appearance of an unsymmetrical band, the maximum intensity being nearer the violet; in the third strip it extends from λ 4280 to λ 4295 and has about the same intensity throughout; in the fourth strip the intensity of its violet edge has decreased, while that of the red edge has increased considerably, giving it the appearance of an unsymmetrical band with the maximum intensity towards the red. In the fifth strip the violet shading from λ 4280 to about λ 4284 has disappeared, leaving a band very nearly symmetrical about λ 4290. It appears, therefore, that we are really dealing with two unresolved bands, one having its center at about λ 4285, and the other at λ 4292.

The band at λ 4275, due to the aqueous solution, decreases in intensity throughout, but more rapidly from the third to the sixth strips than at first. Its position remains the same throughout. As near as the eye can judge, this band has had its intensity reduced to about half-value when the fourth strip is reached, corresponding to 8 per cent. of water in the solution. The alcohol band at λ 4292 also has about 50 per cent. of its final intensity in the same solution.

The band at λ 4760 shows the same kind of a change that we described in some detail above, only the change is much more gradual and easy to follow here. It also shows about equal intensity for the two sets of bands when the amount of water is 8 per cent. of the whole.

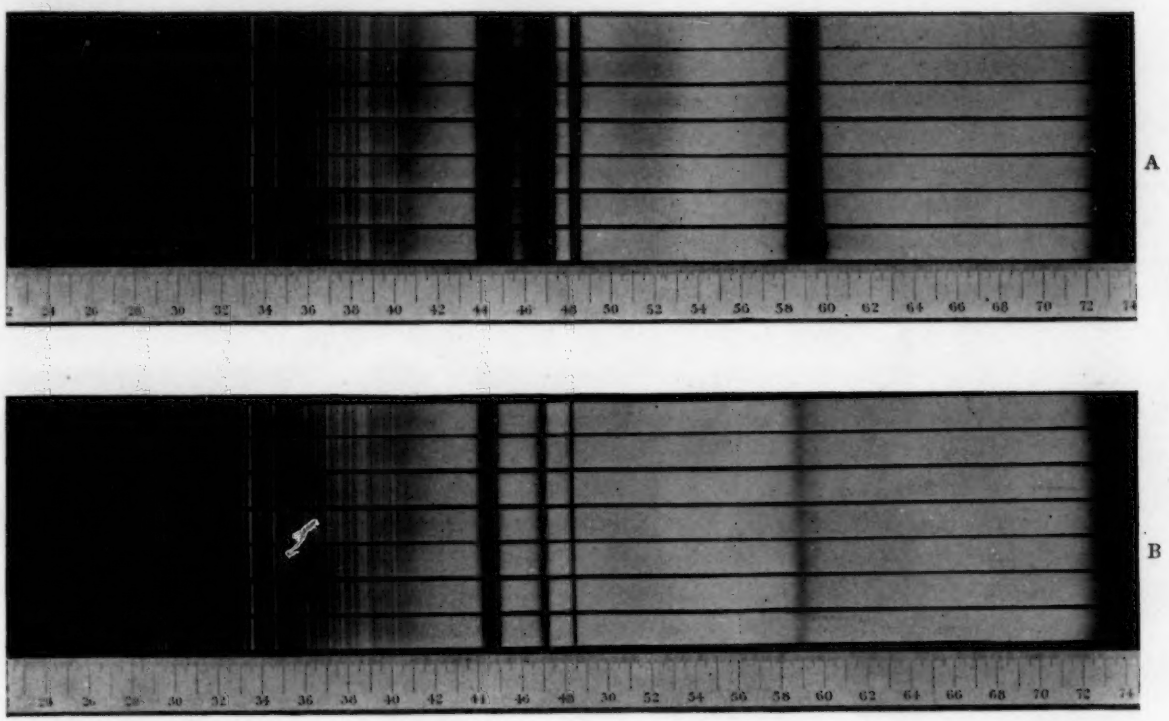
The green and yellow bands are not sufficiently resolved in *a* to allow the change in the individual bands to be followed, and hence these apparently show only a gradual shift towards the red with decrease in the amount of water. In *b*, however, they are sufficiently resolved to enable us to follow the change in each individual band, which, although a little difficult, on account of their large number and the incompleteness of their separation in some cases, may still be done. The change is in every respect the same as we have found for the other bands; namely, those due

to the aqueous solution diminish in intensity and reach about half value in the 8 per cent. aqueous solution, while those belonging to the alcoholic solution increase in intensity, as the amount of water is decreased.

The band in the red near λ 6800 shows the change very well indeed, the "water" band having the position λ 6800, while that pertaining to the alcoholic solution is situated at λ 6860, and hence the two are well separated. Here the point of equal intensity appears to be reached in the solution containing 10⅔ per cent. of water, but this is due to the fact that the alcoholic band has a considerably greater intensity than that due to the aqueous solution, conditions as to concentration and depth of layer being the same. Taking this into account it is seen that this band obeys substantially the same rule as the others.

The change in the band at λ 7325 is more difficult to follow, on account of the small intensity of the photographic action on the less refragible side of this position. The band belonging to the aqueous solution may be seen very clearly even in the strip corresponding to the 2⅔ per cent. water solution, but is of course entirely absent in the alcoholic solution. Its intensity in the 2⅔ per cent. solution, however, seems a little greater than we should expect from the behavior of the other bands, but this is perhaps due to the rather weak photographic action in this part of the spectrum, combined with the great intrinsic intensity of the band. The alcoholic solution transmits light as far as λ 7355 where its spectrum ends abruptly in a band.

Throughout this description we have laid great stress on the fact that on plate 4 the two sets of bands coexist; the bands due to the aqueous solution decreasing, while those belonging to the alcoholic solution increase in intensity with decrease in the percentage of water. We have also called attention to the fact that the two sets of bands have about one-half their full intensity in a solution containing about 8 per cent. of water. This was for a 0.5 normal solution.


A
B

Praseodymium Chloride in Water—Beer's Law.

(See plate 5.)

The concentrations of the solutions used in making the negative for *a*, beginning with the one whose spectrum is adjacent to the numbered scale were 2.56, 1.92, 1.25, 0.85, 0.60, 0.42 and 0.32. For *b* the concentrations were 0.85, 0.63, 0.42, 0.28, 0.20, 0.14 and 0.11; the depths of absorbing layer being, respectively, 3, 4, 6, 9, 13, 18 and 24 mm.

The solutions of praseodymium chloride are all green or yellowish green, only the intensity of the color changing with change in the concentration.

For these solutions Beer's Law holds very exactly, excepting for the extreme ultra-violet absorption in *a*, and the yellow bands in the two or three most concentrated solutions of *a*.

The limits of transmission in the ultra-violet for the most concentrated and most dilute solutions of *a* are, respectively, λ 2720 and λ 2650. The edge is fairly sharp, indicating the presence of a rather intense band. This is also indicated by *b*, where the spectrum ends abruptly at λ 2630, the limit being the same for all the solutions.

The absorption bands shown in *a* are as follows: λ 4380 to λ 4480, strong band with red edge somewhat shaded; λ 4640 to λ 4710, sharp on red side, quite diffuse towards the violet; λ 4800 to λ 4830, sharply defined on both sides; λ 5860 to λ 5950, both edges diffuse; λ 5985, fairly narrow band with diffuse edges. The region between this band and the principal yellow one shows very strong absorption.

b shows the following: λ 4410 to λ 4465, both edges a little diffuse; λ 4685, fairly narrow band, still more diffuse towards the violet, although somewhat shaded also towards the red; λ 4815, narrow band with edges slightly shaded; λ 5900, wide hazy band; absorption not complete even at its middle; λ 5985, rather faint, hazy band.

The greenish tinge of the solutions would suggest that there is considerable general absorption in the red, because the absorption in the yellow is not sufficient to impart any marked color to the solution, and the bands in the violet and blue could only give it a yellow

tint. The negative for *a* does, in fact, show pretty strong general absorption from λ 7100 to the end of the red, but no doubt a spectrophotometric study of the solutions would show general absorption much farther down into the red. The negative for *b* shows no sign of this absorption for very obvious reasons.

Praseodymium Chloride in Mixtures of the Alcohols and Water.

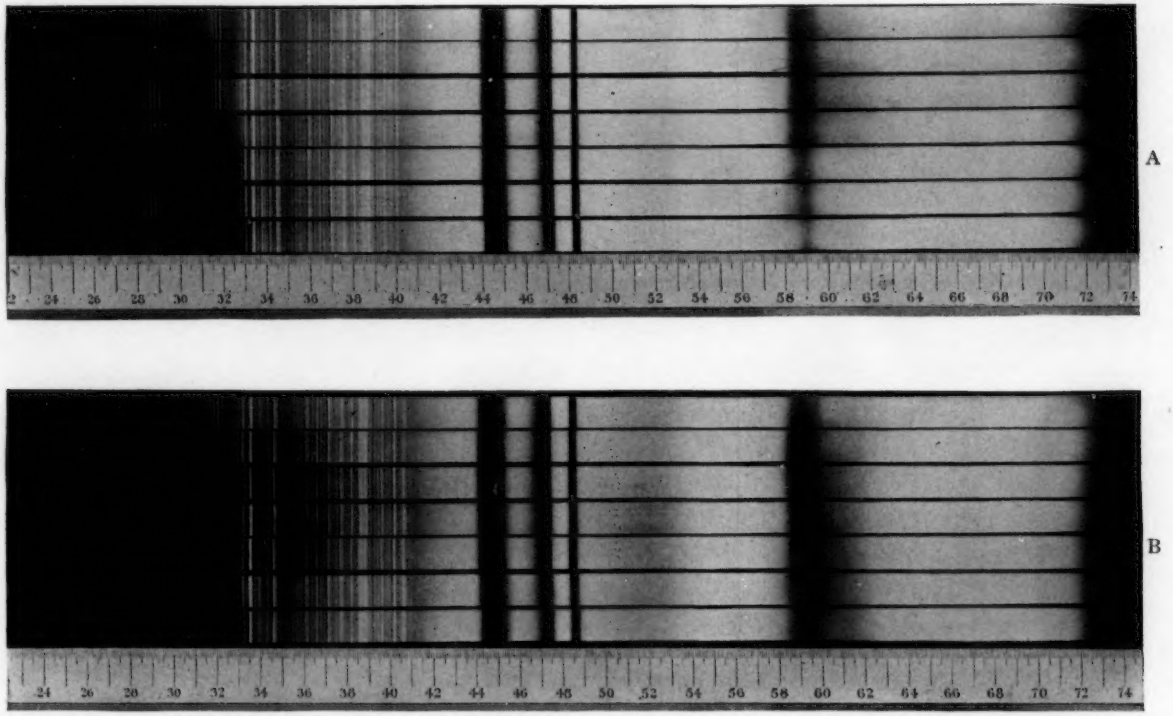
(See plate 6.)

The concentrations of the praseodymium chloride was constant throughout and equal to 0.5 normal. The percentages of water in the solutions, beginning with the one whose spectrum is adjacent to the numbered scale, were 0, $2\frac{1}{3}$, $5\frac{2}{3}$, 8, $10\frac{2}{3}$, $13\frac{1}{3}$ and 16. The depth of absorbing layer was 1.0 cm.

Methyl alcohol was the chief solvent in the solutions pertaining to *a*, while ethyl alcohol was used in the solutions used in making the negative for *b*. The two spectrograms are identical, except for a little greater general absorption in the ultra-violet with the ethyl alcohol.

The most striking feature of the spectrograms is the appearance of the intense absorption band near λ 3000 as the percentage of water is gradually decreased. Only a faint trace of this band is visible with 16 per cent. of water in the solution, and the band is comparatively weak even with only 8 per cent. of water. From this point it increases very rapidly in width and intensity with decrease in the amount of water, until in the pure alcohol solutions its limits (transmission) are λ 2970 and λ 3230, being by far the most intense band in the whole spectrum.

The bands in the violet and blue apparently shift somewhat towards the red, this being, however, due to the fact that the alcohol bands are a little nearer the red end of the spctrum, and that when the percentage of water changes from 16 to 0, the two sets of bands coexist, but are far from being separated. The change is exactly the same in character as the one described in detail in discussing the λ 4760 band in mixtures of alcohol and water for neodymium chloride. The positions of the bands in the solution, containing 16 per cent. of water, are as follows: λ 4390 to λ 4470,


A
B

λ 4660 to λ 4700, λ 4800 to λ 4825. In the solution in pure alcohol they are λ 4410 to λ 4480, λ 4690 to λ 4715, λ 4810 to λ 4840. Hence it appears that the two most refragible bands have a slightly greater width in the aqueous solution, while the λ 4815 band is more intense in the alcoholic solutions.

The bands in the yellow show very well, indeed; the fact that here as in the spectrum of neodymium chloride we have the coexistence of two sets of bands when the water content of a one-half normal solution is in the neighborhood of 8 per cent. The band in the yellow has already been described under Beer's Law, but as the concentration and depth of layer is different here, the following will serve to indicate what the spectrum of the 16 per cent. aqueous solution shows.

Absorption begins at λ 5850 and rises to a maximum at about λ 5900, then decreases to a minimum at λ 5950, from which it again rises to a maximum at about λ 5980, falling off to zero at λ 6000. The solution in pure alcohol shows the following: Weak absorption begins at λ 5800, and continues without material change up to λ 5880, where it falls almost to nothing. At λ 5900 it begins to increase and reaches a strong maximum at λ 5955, falling off gradually to zero at λ 6000. The intermediate solutions show the gradual disappearance of the bands characteristic of the aqueous solution, and the increase in intensity of those belonging to the alcoholic solution as the percentage of water is gradually decreased. The maximum change takes place from the fifth to the third strips, counting from the numbered scale, indicating here as with neodymium chloride that the two sets have about half their normal intensity when the water content of the solution is about 8 per cent., or when the solution contains about ten molecules of water per molecule of the dissolved substance.

Discussion of the Results.

The results established by these plates may be briefly summarized as follows:

1. The absorption spectra of a salt in different solvents are, in general, different.

2. When a salt is dissolved in mixtures of two solvents the

relative percentages of which are varied, there is not a gradual change of one spectrum into the other, but the spectrum given in the mixture is a superposition of the two spectra, the two sets of bands existing together. If the salt is one whose spectrum changes considerably with its state of dissociation, we have in addition to the above phenomena the changes due to the varying dissociation of the dissolved salt produced by the varying composition of the mixture.

A study of all the plates (eighty in number) obtained in this work shows that deviations from Beer's Law is the rule rather than the exception; only a limited number obey Beer's Law even approximately. Beer's Law could only hold in cases where the relative concentrations of the different kinds of absorbers in solution do not change with the dilution, or where the different kinds of absorbers have the same kind of absorption. The first condition is perhaps never realized, while the second is undoubtedly closely approached with such salts as neodymium chloride and praseodymium chloride.

The rule is that the different absorbers have different absorbing powers, and the problem of absorption spectra is to determine which kind of absorbers in solution are responsible for the different bands.

The theory of Ostwald, which would refer absorption in solution mainly to the ions present, has been found to be entirely insufficient to account for the facts established in this investigation.

The other theories which aim to account for the deviations are of two kinds, viz.:

1. Those that assume that the increased absorption in concentrated solutions is due to the formation of aggregates of the molecules of the dissolved substance, or of the molecules and the ions into which they break down in dissociation.

2. Those that assume that the deviation is due to the *formation of solvates,* that is, combinations of the parts of the dissolved substance with the molecules of the solvent.

Now, it has been shown by Hartley and other workers, who have studied the change in the absorption with change in temperature, that the bands which widen with increase in concentration (conditions for Beer's Law assumed to obtain) also widen with

rise in temperature; that is, a rise in temperature produces very much the same effect as increase in concentration. This seems to us pretty conclusive evidence against the theories which are based on the formation of aggregates, for it is well known that the change in the aggregates produced by rise in temperature is *not the same* as that produced by increase in concentration, but *exactly the opposite.*

The theories which assume the formation of solvates are not open to this objection, because it is well known that the change in the solvates produced by rise in temperature is in general the *same* as that produced by increase in concentration. As a solution becomes more concentrated the solvates become simpler and simpler, that is, fewer molecules of the solvent are combined with each part of the dissolved substance. Rise in temperature also breaks down complex solvates into simpler ones. Of course, it does not follow that the solvates of a solution of concentration c_1 at temperature t_1 are *exactly* the same as those in a solution of concentration c_2 at a temperature t_2; since under the changed conditions it may happen that the particular solvates, which were most stable when the conditions were c_1 and t_1, may be less stable than solvates of nearly the same composition at c_2, t_2.

For this reason and also because our work on neodymium and praseodymium salts in mixed solvents *seems almost conclusive evidence in favor of the existence of solvates, we have used the solvate theory as a working hypothesis throughout this work.* That it is not far from being correct is shown by the fact that all the phenomena observed in the great number, about 1,200, of solutions studied, are accounted for without anything but the simplest assumptions in regard to the behavior of the solvates in question.

The most interesting and important results were obtained from the study of the salts of neodymium and praseodymium, especially those of the former. These substances have not only very many absorption bands, but they are remarkably narrow and sharp, and, hence, peculiarly suitable for spectrographic study. The chief experimental results were the following:

1. The absorption spectrum of aqueous solutions of the chloride and bromide of neodymium changes very little with change in con-

centration, and the two are nearly identical, throughout, excepting for the fact that the absorbing power of the bromide appears to be somewhat greater than that of the chloride.

2. Solutions of the salts in non-aqueous solvents give spectra which are not only different for different salts, but the spectrum of any one salt is different in the different solvents. An apparent exception is the spectrum of neodymium or praseodymium chloride in methyl and ethyl alcohols, which are almost exactly alike.

3. When a salt like neodymium chloride is dissolved in mixtures of water and one of the non-aqueous solvents, and the relative amounts of the two solvents in the mixture is varied, no marked change in the spectrum is observed when the amount of water is changed from 100 per cent. to about 15 or 20 per cent. As the amount of water is still further reduced we find that the solution gives a spectrum which consists of a superposition of the spectra belonging to the aqueous and the non-aqueous solutions; the former decreasing in intensity while the latter increases as the amount of water is decreased. The composition of the mixed solvents, which will show the two spectra with about one-half their normal intensity, depends upon the concentration of the salt in solution; and a constant ratio between the number of molecules of water and those of the dissolved salt were indicated by the experiments, this ratio having the value 10.

Praseodymium chloride, dissolved in mixtures of water and methyl or ethyl alcohol, shows in general the same kind of change in the spectrum as neodymium chloride; but in addition there appears in the alcoholic solutions an entirely new band having no analogue in the aqueous solution. In the former this new band in the ultra-violet is by far the most intense in the entire spectrum. It disappears entirely on addition of water, having about half its normal intensity for a half normal solution when the water content of the solvent is about 8 per cent.

These facts seem to us inexplicable on any other hypothesis than the one we have made, namely, that when a salt of one of these elements is dissolved in any solvent, both *the molecules of the salt and the ions formed from these become solvated, that is, they combine with a certain number of molecules of the solvent.* While in

the case of some salts the spectra point to the existence of solvates of varying complexity, in the case of salts of neodymium and praseodymium they indicate rather the existence of one definite hydrate. A more extended study, including the changes in the spectra produced by changes in temperature, may, however, somewhat modify this conclusion.

Granting the existence of solvates all of the facts observed in connection with the absorption spectra of neodymium and praseodymium salts can be readily explained.

PHYSICAL CHEMICAL LABORATORY,
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June, 1908.

PRELIMINARY REPORT UPON A CRYSTALLOGRAPHIC STUDY OF THE HEMOGLOBINS: A CONTRIBUTION TO THE SPECIFICITY OF CORRESPONDING VITAL SUBSTANCES IN DIFFERENT VERTEBRATES.

By EDWARD T. REICHERT and AMOS P. BROWN.

(*Read April 24, 1908.*)

The primary object of this research was to determine whether or not corresponding proteins are identical in different species. Hemoglobin was selected as a favorable substance to begin such a study upon because of its being readily obtained in a state of comparative purity, and, in many cases, readily isolated in crystals. When a sufficient supply of blood was available, it was nearly always possible, by the use of suitable methods, to produce well formed crystals that could be satisfactorily examined and studied by the method adopted. The crystallographic method was chosen because, by its means, differences in substances may be observed that would elude the ordinary methods of analysis employed by the chemist. Moreover, it is comparatively rapid and therefore well adapted to the study of a substance so liable to alteration as hemoglobin. In the method employed it was not even necessary to remove the crystals from the mother liquor for examination. In studying the crystals and measuring the crystallographic constants the petrographic microscope was used, but in the case of these crystals of hemoglobin we have this advantage over the petrographer in his examination of rock sections, in that these crystals are not imbedded in an opaque or semi-opaque matrix, but are in a transparent medium and are usually isolated from each other. Moreover, hundreds and often thousands of crystals are open to observation in a single slide, and these present almost all possible orientations, allowing the optical characters to be determined with much greater accuracy than

is usually the case with minerals in rock sections. Measurements within the limit of error of the instrument could frequently be obtained, and, as various orientations were available, the results of the angular measurements often furnished complete data for the calculation of the axial ratios. The crystals examined were usually complete and often geometrically perfect, so that the symmetry and crystal habit could be determined as readily as in the case of ordinary mineral substances occurring in isolated crystals.

A chemical substance, possessing a rational composition, tends to arrange its parts in an orderly manner so that a definite structure is assumed, which results in a definite external form. This is so universally true that the crystalline condition is the normal one for matter of definite composition. Differences of crystalline form hence indicate differences of substance; and, by the crystallographic method of investigation, obscure differences, such as those between isomerides, may readily be detected.

Photographic records of the crystals were secured and upwards of 2,500 negatives have been made. The hemoglobins of more than one hundred species have been examined and data secured in regard to their crystals. From a study of these records certain facts stand out very prominently.

1. *The Constancy of Generic Characters in the Crystals.*—The crystals of the species of any genus belong to a crystallographic group. When their characters are tabulated, they at once recall the crystallographic groups of minerals. The crystals of the genus *Felis* form an isomorphous group; as strictly isomorphous, in fact, as the group of the rhombohedral carbonates among minerals. The genus *Canis* is even more strictly isomorphous, but the crystals of hemoglobin from the two genera are perfectly distinct, the one from the other.

As an example of the individuality of these generic characters the following may be cited: A sample of blood, marked as that of a certain species of baboon was received from one of our Zoölogical Gardens. Upon making preparations and examining the crystals, it was at once evident that they did not correspond to any species of

baboon thus far examined, nor did they show the characters of the genus *Papio*. They were identified by their crystallographic characters as belonging to the cats (genus *Felis*) but not to any species that we had examined up to that time. Inquiry at the Zoölogical Garden from which the blood was received showed that the animal recorded as being subjected to a *post-mortem* examination on the date when the blood was collected was a species of the genus *Felis*, but not one of which we had previously examined the blood. Other similar cases of incorrect labelling of specimens were detected, in which the wrongly labelled blood was one that had been examined and the species known from other specimens.

2. *Specificity in the Crystals of a Genus.*—The crystals of the different species of a genus, when they are favorably developed for good measurement, can usually be distinguished from each other by definite differences of angle, etc.; while preserving their isomorphous character as belonging to a definite genus. In cases where, on account of difficulty of measurement, the differences cannot be given a quantitative value, variations in the habit of the crystals and in their mode of growth will often show specific differences.

3. *The Occurrence of Several Types of Crystals of Oxyhemoglobin in Many Species.*—In some species the oxyhemoglobin is *dimorphous* (crystallizing in two systems or with two axial ratios), in other cases even *trimorphous*. Where several types of crystal occur in this way in the species of any genus, the crystals of *each type* may be arranged in an isomorphous series. In other words, certain genera are isodimorphous or isotrimorphous.

4. *The Constant Recurrence of Certain Angles, Plane or Dihedral, in the Oxyhemoglobin, Hemoglobin and the "Methemoglobins" of Various Species, even when these Species are Widely Separated Zoölogically and when their Crystals Belong to Various Crystal Systems.*—This appears to indicate a common substance in hemoglobins or a common structure in the various hemoglobin molecules.

5. *The Constant Recurrence of Certain Types of Twinning in the Hemoglobins, and the Prevalence of Mimosie in these Crystals.*—This also indicates a common structure in the various hemoglobin molecules.

6. *Differences between Oxyhemoglobin and Reduced Hemoglobin in Certain Species.*—Undoubted differences between the crystals of these two substances in the same species have been observed.

We have gathered additional evidence that other corresponding proteins, as well as certain fats and carbohydrates, will be found to exhibit similar specificities.

UNIVERSITY OF PENNSYLVANIA.
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INFLUENCE OF PRESERVATIVES AND OTHER SUBSTANCES ADDED TO FOODS UPON HEALTH AND METABOLISM.

By HARVEY W. WILEY, M.D.

(*Read April 25, 1908.*)

In connection with studies of food adulteration, which have been conducted during the past twenty-five years under my direction in the Bureau of Chemistry, frequent evidence was obtained of the addition of certain preserving agents and coloring matters to food products. These bodies are not of the character known as condimental; on the contrary, as a rule, they possess neither appreciable taste nor odor in the quantities in which they are employed.

In so far as preservatives are concerned, therefore, the consumer would have no certain knowledge of their presence, and in respect to coloring matters, he would likewise be ordinarily deceived, since such coloring matters are often used to imitate the natural tints found in food products. Thus there would be practiced upon the consumer a fraud in that in the purchase and consumption of foods he was buying and consuming articles which are distinctly not foods and the presence of which is a just cause of suspicion.

The use of chemical preservatives and artificial colors in foods is of quite recent date. I think I may say with safety that if one could go back thirty, or at most, forty years, he would find a food supply practically free, both from chemical preservatives and artificial colors. The rapid development of organic and tinctorial chemistry during the past forty years has made it possible to offer to manufacturers chemical preservatives of high potency, and colors of great beauty and persistence, at prices which make it entirely possible to use them freely in food products. Inasmuch as the use of these bodies, whatever the claims may be in regard thereto, has for its chief purpose either to cheapen the product itself or to sell

it at a higher price than it really should command, it is evident that unless the pecuniary conditions attending the use of these bodies were favorable they would not be employed.

When the claims which are made by manufacturers respecting the use of these substances are carefully considered, we find that most of them are without foundation. In regard to the supposed general preference for artificial color, I would say that an experiment performed on a large number of totally unbiased people has convinced me beyond any reasonable doubt that the great majority of American consumers would prefer uncolored foods. The experiment mentioned was made on about sixty different men during a period of five years to determine whether or not they preferred an artificially colored food or one in its natural tint. Butter, which is perhaps the one food product most universally colored in this country, was used. The subjects on whom the experiment was tried had been in the habit of using nothing except colored butter, hence, if there was any prejudice existing in their minds it must have been in favor of the article which they had constantly consumed. Moreover, the test was made in the winter time when the uncolored butter has the least tint of the whole year, being almost white. No attempt was made to inform the men of the nature of these products. The natural butter and the colored butter were moulded in the same forms and placed upon the same plate, and offered without comment of any kind. At first very few of the men would do more than look at the uncolored butter. A very common expression was, "This is oleomargarine." A few made a trial of its properties. Little by little, without any propaganda of any kind, the whole attitude of these men changed. In the course of four or five months nine tenths of them were using the uncolored butter and they expressed a most decided antipathy to the use of the colored butter when at certain times the supply of the uncolored butter was exhausted.

I believe that this completely refutes the arguments of those who claim that they color butter to meet the demand of the consumer. In point of fact, the color in butter has been almost from the first a fraudulent process. It is a common belief that the best butter of the year is produced during the early spring months, and especially

in June, when the cows have access to the succulent pastures. During this time, owing to the oxidation of the chlorophyll of the grass, a xanthophyll is produced, imparting to the cream a rich golden, or yellow, tint which is, of course, perpetuated in the butter. During the winter months, when the chlorophyll is withdrawn practically from the diet of the cow, this natural coloring matter is absent. The use of the artificial color, therefore, is to simulate for winter butter the color of the butter in June, and thus to conceal what is at least believed to be inferiority.

Again, in experimental observations of a less extended character, I have found that the American consumer does not prefer his foods preserved with chemical preservatives. In a large number of instances which have come under my own personal observation the consumer has stopped eating an article as soon as he has found that it contains a chemical powerful enough to inhibit fermentative action. The users of chemical preservatives, however, do not as a rule claim that they use them at the demand of the consumer. A careful study of manufacturing data made by one of the most conscientious manufacturers in the West shows that it costs more to make a food product without a preservative than it does with a preservative. In very extensive practical experiments on tomatoes this manufacturer found that it was necessary to charge from fifteen to twenty cents more for ketchup per case made without a preservative than with a preservative. Thus I think it is well established by this experimental study that the real reason which the manufacturers have for using chemical preservatives is to cheapen the cost of production. This of itself would be a most worthy object, because presumably the cheapening of the cost of production would lower the price to the consumer. If, therefore, a food product of equal nutritive value and equal wholesomeness could be produced with the aid of chemical preservatives, such a process should meet with the approbation of all. But a very serious problem of a different kind is presented here. A chemical preservative is effective usually by reason of its inhibitive action on fermentation. Very extensive studies of this action of chemical preservatives have led to the general conclusion that while these bodies inhibit the fermentative action giving rise to the ordinary

evidences of decay and putrefaction, and, as a rule, stop most effectively those fermentations which produce alcohol and carbon dioxid, they do not have the same restrictive influence on those processes resulting in the general degradation and decay of organic matter, due chiefly to that class of chemical reactions which is represented by the term hydrolysis. In other words, the ferments which break down, for instance, nitrogeneous tissues into more soluble and finally more dangerous forms of combination, are not so particularly inhibited as is the first class of ferments mentioned.

This fact might well be used, however, as a justification of the employment of chemical preservatives, since if they prevent the ordinary processes of fermentation which produce evident indications of decay and putrefaction, it might be held that they would not interfere with that other class of fermentations or hydrolytic processes peculiarly exercised by the digestive ferments. It will probably not be contested at the present time that there is some justification for this plea, since it has been well established that an amount of a preservative which will for instance prevent alcoholic fermentation will not interfere in anything like so serious a manner with the action of such ferments as the diastatic ferments of the saliva, of the stomach, and of the pancreas. On the other hand, it is well established that in any notable quantities these preservatives do interfere with even the latter class of ferments.

But the problem which is of most importance in this connection is, What is the chief effect of these preservatives upon the health of those who constantly use them and upon the metabolism resulting from the normal functions of the body? To answer this question, there was begun in an experimental way in the Bureau of Chemistry, under my direction, a few years ago, a series of studies having for their purpose the elucidation of this problem. The general plan of the experimental work was extremely simple. It contemplated the selection of a number of young men between the ages of twenty and thirty, in excellent health, who had suffered from no serious disease in the immediate past, who were of steady habits, who were not addicted to the use of alcohol, and whose character was such as to warrant especial confidence and trust in their veracity and general conduct. Such young men evidently are to be

found among those who pass the examinations for the civil service of the United States. In these examinations the very qualities which were looked for in the young men in question must be present or they could not receive the vouchers for character and conduct which are necessary to entitle them to compete in the examinations. These young men were subjected to a careful physical examination similar to that exercised upon those who apply for policies in life insurance companies. This examination showed them free from organic diseases and not to have suffered within a year, usually not at all, from any serious disturbance of health. The subjects were placed upon their honor, by a formal pledge, that they would obey all the rules established for the experimental work and abstain from any form of food and drink except that offered in the regular course of the investigation. Those who used tobacco, tea and coffee were permitted to continue to do so in the regular manner so as not to change the habits of their previous daily life. They were also limited by their pledge to a regular course of exercise which they undertook to follow without variation, and also regular hours of work and sleep. As a justification of the faith and confidence reposed in these young men I think it is sufficient to say that, although during the five years of the experiments we have had about sixty young men under observation, only three have been found to have violated their pledges.

The subjects so selected were first placed on a generous diet of the kind and character to keep them in equilibrium; that is, to maintain the weight of their body without notable changes. The part of the experiment devoted to this purpose was known as the "fore period." Each one was allowed to determine, within certain limits, the character of the diet from the foods offered; that is, a relative amount of meat, bread, potatoes, butter, milk, coffee, tea, etc., to suit his taste and to conform with his previous habit of life. Only in those cases where an excess of some particular kind of food seemed to be preferred was any restriction placed upon this matter. This fore period, therefore, enabled us to determine the magnitude of the ration which would preserve the body equilibrium and presumably be in entire conformity with the normal digestive functions.

The study of the food ingested and of the excreta secured established a chemical control whereby it would be easy to determine any variation in the quantity of food consumed should any of the young men attempt to evade the conditions of their pledge. Having thus established the normal conditions of the body and ascertained the normal metabolic processes, there was introduced into the same ration varying quantities of the preservative which was to be studied. It was thus evident that any change taking place in health or metabolism could be due only to the one factor which was varied in the method of life, namely the injection of the chemical preservative. This period, during which a drug was used, was known as the "preservative period," and lasted, according to circumstances, from twenty to sixty days, depending upon the character and magnitude of the effects produced. As soon as any decided disturbance of health was produced, clearly traceable to the administration of the preservative, its future use was discontinued since it was not the purpose to seriously or permanently affect the health of the subject, but only to secure positive diagnostic data. Then followed an "after period," during which the chemical, or drug, was withdrawn from the food and the normal ration continued as in the fore period, the object being to correct, if possible, any disturbances of metabolism which had been produced and restore the subject again to normal conditions of health and digestion and also to study the after effects of the preservative should such persist. This period of observation was called the "after period." Thus each series of experimental investigations were divided into these three periods.

During the progress of the experiment the following substances were added to the foods for the purposes mentioned above: Boric acid, borates, salicylic acid, salicylates, benzoic acid, benzoates, sulphurous acid, sulphites, formaldehyde, sulphate of copper and potassium nitrate. There is given in the accompanying table a condensed statement of the effects which were produced in these various cases. It is not the purpose of this paper to go into the experimental detail of this matter. The amount of chemical analysis incident to this study was enormous. A great many chemists gave their entire time during the whole period of observation to these analytical problems,

HEALTH AND METABOLISM AS INFLUENCED BY PRESERVATIVES.[1]

Administration of Preservatives.				Medical and Clinical Notes.	Body Weight	Urine Determinations.				Red Blood Corpuscles.	Metabolism.		(Percentage of Total Excretion.)		
Substance.	Number of Days.	Daily Amount Grams.	Total (Maximum). Grams.			Volume.	Albumin.	Acidity.	Microscopic Bodies.		Nitrogen.	Phosphoric Acid.	Sulphur.	Fat.	Solids.
Borax	(3 series)	0.5–3.0	79.5 (minimum	Loss of appetite, nausea, headache, depression	—	o	—	+	o	o	—	+	.....	o	+ (very
Boric acid	30 to 70	0.5–5.0	13)		—	o	+	+	o	o					small)
Salicylic acid and salicylates	30	0.21–2	30.85	Hunger, slight headache, and abdominal pain; symptoms not general.	—	o	o (Special study)	o (Special study +)	+	+	Total — (slight) Urine +	—	—	—	Total — Urine +
Sulphurous acid	20	0.171 –0.4	6.856	Headache, dizziness, pain in stomach; weakness; depression.	o	+ (more marked)	+ (slight)	+	+	— (very marked)	+ (especially in feces)	Total + Urine —	Total + Feces—	+ (very slight)	+
Sodium sulphite		0.223 –0.762	12.565		—	+	+ (slight)	+ (more marked)		— (very marked)	+ (especially in feces)	Total + Urine —	Total + Feces—	+ (very slight)	+
Benzoic acid	20	1–2.5	35[2]	Nausea, headache, lassitude.	—	— (slight)	.....	.....	+ (slight)	—	+	+	+	— (very slight)	+ (very slight)
Benzoates					—	— (slight)				+					
Formaldehyde	15	0.1–0.2	2.5	Headache, abdominal pains; sometimes nausea and rash.	—	+	o (+ very few cases)	o (— very few cases)	+ (very slight)	— (not conclusive)	—	Total + Feces —	—	— (very slight)	Total — Urine +
Copper sulphate	19	0.05 –0.15	1.9	Pains in stomach and abdomen; nausea, indigestion; headache; nervousness.	— (very slight)	o	o	o	+ (slight)	—	Total — (Feces +)	— (marked)	Total o (Feces —)	— (slight)	—
Potassium nitrate	60	0.15 –.60	12	Slight headache, pains in epigastrium.	— (slight)	— (Summer)	o	+	+	+	—	— (marked)	+ (slight)	.....	.....

[1] Plus sign signifies a gain, minus sign a loss, and zero practically no change in the preservative period as compared with the fore period.

[2] Only three subjects took the maximum amount.

and in addition to that a number of calculators were employed to tabulate, classify, and average the data. The experimental data which were obtained are published in Bureau of Chemistry Bulletin 84, which when completed will contain the entire series of studies. Part I, of Bulletin 84, is devoted to the detailed study of the effect of borates and boric acid upon health and metabolism. This part of the Bulletin consists of 477 pages. Part II is devoted to the study of salicylic acid and salicylates and contains 283 pages. Part III contains the data relating to sulphurous acid and sulphites, and contain 281 pages, making a total of published matter of 1,041 pages.

CHANGES IN THE URINARY NITROGEN AND SULPHUR COMPOUNDS.[1]

Preservative.	Dose. [Grams per Day.]	Urea.	Uric Acid.	Kreatinin.	Ammonia.	Xanthin.	Neutral.	Total Sulphates.	Inorganic Sulphates.	Ethereal Sulphates.
Borax	0.5 -3.0	.....	.....	.....	.....	.....	.....	.....	.....	.....
Boric acid	0.5 -5.0	.....	.....	.....	.....	.....	.....	.....	.....	.....
Salicylic acid and salicylates	0.21-2.0	—	—	+	—	o	+	+	+	+
Sulphurous acid	0.17-0.4	—	—	o	—	o	+	+	+	+
Sulphites	0.22-0.76						+	+	+	o
Benzoic acid	1.0 -2.5	.....	.....	.....	.....	.....	—	+	+	o
Benzoates	1.0 -2.5	.....	.....	.....	.....	.....	—	+	+	o
Formaldehyde	0.1 -0.2	—	.....	.....	.....	.....	o	o	o	o
Copper sulphate	0.05-0.15	—	+	.....	.....	+	+	—	o	—
Potassium nitrate	0.15-0.6	—	o	o	—	.....	—	o	+	o

The data relating to benzoic acid and benzoates are in press. These data, together with those relating to the other parts of the study which have been completed and submitted for publication, will make a volume of approximately 2,100 pages. All that I can give in this paper will be the general conclusions relating to each part of the study.

CONCLUSIONS.

Boric Acid and Borates.—In the consideration of the action of preservatives of a mineral nature, such as borax and boric acid, it must be remembered that the animal as well as the plant possesses

[1] Minus and plus signs indicate decreased or increased total excretion in preservative period as compared with the fore period.

a certain mineral hunger. In other words, mineral substances play a double role in animal and plant nutrition: First, they may serve as real foods, necessary to the formation and nutrition of the tissue. In the animal economy this is especially true of phosphoric acid and lime. In the second place, they are necessary to the functional activity of the various organs of the body, irrespective of any part they may take in direct nutrition.

The necessity of saline solutions in the blood is known to every physician and physiologist. If the blood were deprived of all of its saline constituents the circulation would be impeded, restricted, or stopped, and death would result. In cases of collapse in disease saline injections in the blood are often used as a restorative measure. These salts in solution stimulate the heart's action and undoubtedly are active in the osmotic operations of the cells. This is one of the facts which show the intimate relation existing between physical chemistry and physiology.

Common salt is the most frequent and most abundant of the saline constituents of the blood, but the alkalinity of the blood is not due of course to the common salt, which is a neutral substance. The existence of alkaline carbonates or other alkaline salts is necessary to the vital functions. While it is true that the digestion in the stomach takes place in an acid solution, it is likewise true that any excessive acid must be neutralized and enough of alkali added in the small intestine in order that the further digestion of the food may properly take place. That saline bodies other than common salt or the alkaline carbonates may be useful, however, in the performance of the vital functions cannot be denied, though it might be difficult to demonstrate their absolute necessity. Hence the introduction of saline bodies, which may or may not be of an antiseptic character, may, within certain limits, have a favorable influence upon health and digestion. At the same time it should not be forgotten that all excess of such bodies imposes upon the excretory organs an additional burden, which, while it might not impair their efficiency even for a number of years, might finally produce a condition of exhaustion which would be followed by serious consequences. Especially is this remark true of the kidneys, which appear to be a

general clearing house for all the surplus of saline matters, ingested in the foods.

The most interesting of the observations which were made during the progress of the experiments was in the study of the direct effect of boric acid and borax, when administered in food, upon the health and digestion. When boric acid, or its equivalent in borax, is taken into the food in small quantities, not exceeding half a gram (7½ grains) a day, no notable effects are immediately produced. The medical symptoms of the cases, in long-continued exhibitions of small doses or in large doses extending over a shorter period, show in many instances a manifest tendency to diminish the appetite and to produce a feeling of fullness and uneasiness in the stomach, which in some cases results in nausea, with a very general tendency to produce a sense of fullness in the head, which is often manifested as a dull and persistent headache. In addition to the uneasiness produced in the region of the stomach there appear in some instances sharp and well-located pains, which, however, are not persistent. Although the depression in the weight of the body and some of the other symptoms produced persist in the after periods, there is a uniform tendency manifested after the withdrawal of the preservative toward the removal of the unpleasant sensations in the stomach and head above mentioned.

The administration of boric acid to the amount of 4 or 5 grams per day, or borax equivalent thereto continued for some time, results in most cases in loss of appetite and inability to perform work of any kind. In many cases the person becomes ill and unfit for duty. Four grams per day may be regarded, then, as the limit of exhibition beyond which the normal man may not go. The administration of 3 grams per day produced the same symptoms in many cases, although it appeared that a majority of the men under observation were able to take 3 grams a day for a somewhat protracted period and still perform their duties. They commonly felt injurious effects from the dose, however, and it is certain that the normal man could not long continue to receive 3 grams per day.

In many cases the same results, though less marked, follow the administration of borax to the extent of 2 grams and even of 1 gram per day, although the illness following the administration of

borax and boric acid in those proportions may be explained in some cases by other causes, chiefly grippe.

The administration of borax and boric acid to the extent of one half gram per day yielded results markedly different from those obtained with larger quantities of the preservatives. This experiment, Series V, conducted as it was for a period of fifty days, was a rather severe test, and it appeared that in some instances a somewhat unfavorable result attended it. On the whole, the results show that one half gram per day is too much for the normal man to receive regularly. On the other hand, it is evident that the normal man can receive one half gram per day of boric acid, or of borax expressed in terms of boric acid, for a limited period of time without much danger of impairment of health.

It is, of course, not to be denied that both borax and boric acid are recognized as valuable remedies in medicine. There are certain diseases in which these remedies are regularly prescribed for both internal and external use. The value which they possess in these cases does not seem to have any relation to their use in the healthy organism except when properly prescribed as prophylactics. The fact that any remedy is useful in disease does not appear to logically warrant its use at any other time.

It appears, therefore, that both boric acid and borax, when continually administered in small doses for a long period or when given in large quantities for a short period, create disturbances of appetite, of digestion, and of health.

Salicylic Acid and Salicylates.—In the conclusions based upon the general observations the same conservatism must be observed and the same general reservations made as are found in Part I concerning boric acid and borax. While, as described in the borax report, the attempt has been made to control as far as possible, all the conditions of the experimental work, the difficulties attending the task are so enormous that it is not possible that complete success should be secured. There has, however, been no attempt made to discriminate in the choice of data, all the observations being recorded and the discussion of the individual data based upon the tabular statements being without prejudice and without bias. The general assumption has been made, as in the previous cases, that,

by reason of the regular habits of life which were imposed upon the subjects, the amount of energy developed and the quantity of nourishment expended therein are reasonably constant throughout the experimental period. If these factors vary, as they necessarily must to a certain degree, it is evident that they vary uniformly above or below the average, and hence these variations could not possibly produce any notable effect upon the final result.

There has been a general consensus of opinion among scientific men, including the medical profession, that salicylic acid and its compounds are very harmful substances, and the prejudice against this particular form of preservative is perhaps greater than against any other material used for preserving foods. This is due not only to the belief in the injurious character of salicylic acid, but perhaps is especially due to the fact that it has in the past been so generally used as an antiseptic. That salicylic acid should be singled out especially for condemnation among preservatives does not seem to be justified by the data which are presented and discussed in this bulletin. That it is a harmful substance, however, seems to be well established by the data taken as a whole, but it appears to be a harmful substance of less virulence than has been generally supposed. There is no doubt of the fact that salicylic acid is a drug which is often indicated in diseases well established and also perhaps in certain conditions which, while verging on disease, might still be regarded as a state of health. But the administration of salicylic acid as a medicine should be controlled exclusively by the medical profession, and while it is a remedy well established in the Pharmacopœia and especially prized for its effect upon rheumatism and gout, it does not seem that there should be any warrant in this fact for its promiscuous use in foods, even if it were harmless.

The data show very clearly that salicylic acid and salicylates appear to exert an exciting influence upon the activities which take place in the alimentary canal, stimulating the organs to greater effort, and this stimulation leads at first to increased solubility and absorption of the foods which are introduced into the stomach. In the light of the data which are exhibited salicylic acid may be said to increase the solubility and absorption of the food in the alimentary

canal, so that larger parts of the nutrients taken into the stomach actually enter the circulation.

The data which show the effect just noted also indicate that the general effect upon the system is depressing, in that the tissues are broken down more rapidly than they are built up, and thus the normal metabolic processes are interfered with in a harmful way. The administration of the salicylic acid is attended by a gradual decrease in the weight of the subjects, although the quantity of food elements administered during the preservative and after periods is slightly increased, which fact, together with the greater degree of absorption of the food elements, should have resulted in a slight increase in weight. This increase in weight, however, does not occur, and the disturbing influence of the salicylic acid upon metabolism, although not very great, is specifically demonstrated.

The final conclusion in this matter, therefore, is that the unenviable position which salicylic acid has heretofore held among preservatives, in being regarded as the most injurious of all, is to a certain extent undeserved. Like other ordinary preservatives, it is not one which can be classed as a poison in the usual sense of the word. When used as a medicine in many cases of derangement of health it is like the other chemical preservatives, often highly beneficial when properly prescribed by a competent physician. It is when used in the food at first an apparent stimulant, increasing the absorption and solubility of the common food elements from the alimentary canal. It soon, however, loses its stimulating properties and becomes a depressant, tending to break down the tissues of the body more rapidly than they are built up. It disturbs the metabolic processes, in most cases producing conditions which are not normal and which, apparently, are not beneficial. It has a tendency to diminish the weight of the body and to produce a feeling of discomfort and malaise, which, while not marked, is distinctly indicative of injury. In some cases these symptoms of malaise approach illness, and while not always diagnostic are sufficiently common to point unmistakably to the salicylic acid as their origin. It places upon the excretory organs, especially the kidneys, an additional burden which they are not able to bear and which cannot possibly result in any good, but on the contrary must necessarily finally result in injury, though per-

haps with the use of very small quantities of the preservative these organs would continue to perform their function for many years before finally breaking down.

This work is offered as an unbiased study of all the data recorded, both of those which appear to be in favor of the use of salicylic acid and those which appear to be against its use, and leads to the inevitable conclusion that salicylic acid is a substance which, when added to foods even in small quantities, exerts a depressing and harmful influence upon the digestion and health and the general metabolic activities of the body. Further, there appears to be no necessity for its use, as food can be preserved in unobjectionable ways without its aid. Its indiscriminate use would tend to carelessness in the quantities employed, thus increasing the dangers to which the consumer is subjected. Also its use in the preservation of foods tends to induce carelessness and indifference on the part of the manufacturer, as when a chemical antiseptic is employed many of the processes necessary to the proper selection, cleaning, and preservation of foods may be omitted.

The addition of salicylic acid and salicylates to foods is therefore a process which is reprehensible in every respect, and leads to injury to the consumer, which, though in many cases not easily measured, must finally be productive of great harm.

Sulphurous Acid and Sulphites.—From a careful consideration of the data in the individual cases and the summaries of the results, it appears that the administration of sulphurous acid in foods, either in the form of sulphurous acid gas in solution or in the form of sulphites, is objectionable and produces serious disturbances of the metabolic functions and injury to health and digestion. This injury manifests itself in a number of different ways, both in the production of clinical symptoms which indicate serious disturbances, malaise, or positive suffering, and also by inducing certain changes in the metabolic processes which are not manifested in the way of ordinary clinical symptoms, and are only detected by careful chemical and microscopical study of the excretory products. It can safely be said from the evidence adduced that the administration of sodium sulphite and sulphurous acid as above indicated produces a marked influence of an unfavorable character on metabolism. As

a result of this action an assimilation of food materials containing organic phosphorus is retarded, while there is evidence of increased sulphur katabolism. The sulphur balance sheets show what an immense burden has been added to the already overworked kidneys, which are called upon in this case to remove nearly all, if not quite all, of the added sulphur from the body, previously converted, in great part to sulphuric acid. It is not possible that placing upon the kidneys this increased work of excreting sulphur can result in anything but injury. The fact that the microscopic crystalline and amorphous bodies in the urine are increased in number under the influence of the added sulphur, is another indication of the extraordinary demands made upon the kidneys in such circumstances.

This increase is interesting in respect of the effect which the continued exhibition of sulphurous acid must eventually have upon the structure of the kidney. It is reasonable to suppose that the continued use of a body which produces such results would cause lesions of a histological character which eventually would develop conditions which would give serious apprehension. In the nature of these experiments it was not possible to examine the organs of the body histologically and hence the above conclusion is only based upon experience of a similar character where the organs in question have been subject to such examinations. While there might be no distinguishable lesion of the kidneys produced during a period of twenty or thirty days, or even longer, it is plain that sooner or later lesions of a very serious character producing organic diseases, possibly of an incurable type, would be induced. The further observation that there is a marked tendency to the production of albuminuria, although of an incipient character, is an indication of the unfavorable results of the administration of the sulphurous acid. It is, therefore, evident that by increasing the burden upon the excretory organs, the administration of sulphur in the form mentioned is highly detrimental to health.

All of these tendencies cannot be interpreted as being other than of a decidedly harmful nature. Another effect which the administration of the sulphur produced, and one of a more serious character still, is found in the impoverishment of the blood in respect of the number of red and white corpuscles therein. The administration

of a substance which diminishes by a notable percentage these important component particles of the blood must be regarded in every sense as highly prejudicial to health. Some of the most important functions of the blood, as has been well established by careful physiological studies, are intimately connected with the number and activity of both the red and white corpuscles. The bleaching effect of the sulphurous acid upon the color of the blood is a matter of less consequence and no great effect is produced upon the hemoglobin, but the diminution of the number of red and white corpuscles is a matter of serious concern.

The variations of the metabolic processes from the normal, as indicated in this series of experiments, were never of a character favorable to a more healthy condition of the system, but, on the other hand, all these variations, in so far as the effect of the changes could be distinguished, are of a prejudicial character. There is no evidence whatever that the sulphur added to the foods in the form of sulphurous acid, or sulphites, takes any part in the nutrition of the tissues of the body containing sulphur, namely, the proteids; hence, no claim of food value can be established for these bodies. The evidence all points to the fact that they are purely drugs, devoid of food value, having no favorable effects upon the metabolic processes, but, on the other hand, exerting deleterious and harmful effects. The conclusion, therefore, is inevitable that, as a whole, the changes produced in metabolic activity by the administration of sulphur in the forms noted above in the comparatively short time covered by the experiments are decidedly injurious.

The verdict which must be pronounced in this case is decidedly unfavorable to the use of this preservative in any quantity or for any period of time, and shows the desirability of avoiding the addition of any form of sulphurous acid to products intended for human food.

Benzoic Acid and Benzoates.—From a careful study of the data in the individual cases and of the summaries of the results, it is evident that the administration of benzoic acid, either as such or in the form of benzoate of soda, is highly objectionable and produces a very serious disturbance of the metabolic functions, attended with injury to digestion and health.

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As in the case of boric acid, salicylic acid, and sulphurous acid, this injury manifests itself in a number of different ways, both in the production of unfavorable symptoms and in the disturbance of metabolism. These injurious effects are evident in the medical and clinical data which show grave disturbances of digestion, attended by phenomena which are clearly indicative of irritation, nausea, headache, and in a few cases vomiting. These symptoms were not only well marked, but they were produced upon healthy individuals receiving good and nourishing food and living under proper sanitary conditions. It is only fair to conclude, therefore, that under similar conditions of administration of benzoic acid or benzoate of soda in the case of weaker systems, or less resistant conditions of health, much more serious and lasting injury would be produced.

It was also noticed that the administration of benzoic acid and benzoate of soda was attended with a distinct loss of weight, indicative of either a disturbance of assimilation or an increased activity in those processes of the body which result in destruction of tissue. The production of a loss of weight in cases of this kind must be regarded as indicative of injurious effects.

The influence of the benzoic acid and benzoate of soda upon metabolism was never of a character indicative of a favorable change therein. While often the metabolic changes were not strongly marked, such changes as were established were of an injurious nature. It is evident that the administration of these bodies, therefore, in the food tends to derange metabolism in an injurious way.

An important fact in connection with the administration of these bodies is found in the efforts which nature makes to eliminate them from the system. In so far as possible the benzoic acid is converted into hippuric acid. There is a tendency usually manifested, however, to retain the benzoic acid in the body for a notable length of time, and this is much more marked in the case of benzoate of soda than in the case of benzoic acid.

While the administration of both these bodies, therefore, is undoubtedly harmful, the injurious effects are produced more rapidly in the case of benzoic acid than they are in the case of benzoate of soda; the data, however, will show that the total harmful effect produced in the end is practically the same in both cases, hence there

appears to be no reason for supposing that the administration of the preservative in the form of benzoate of soda can be justified by any argument relating to the less injurious effect thereof upon health.

The occurrence of microscopic bodies in the urine is undoubtedly increased under the administration of benzoic acid in all its forms, thus showing conclusively the tendency to stimulate the destructive activities of the body.

Coming to the final consideration of all these different phases of the subject, there is only one conclusion to be drawn from the data which have been presented and that is that in the interests of health both benzoic acid and benzoate of soda should be excluded from food products. This conclusion is reached independently of any consideration of the conditions which it is alleged surround the processes of manufacture and which result in the demands of manufacturers to be allowed to continue the use of this body. This is a subject which must be discussed from an entirely different point of view and has no bearing whatever upon the general conclusions which have been reached, namely, that both benzoic acid and benzoate of soda are bodies which, when added to foods, are injurious to health.

Formaldehyde.—A general study of all the data leads to the conclusion that the admixture of formaldehyde with food is injurious to health, even in the case of healthy young men. It is fair to conclude, therefore, that in the case of infants and children the deleterious effects would be more pronounced. The metabolic functions are disturbed in a notable way, both by the retardation of the nitrogen and sulphur metabolism, and the acceleration of phosphorus metabolism. There seems to be a tendency to an increased absorption from the alimentary canal, especially in the cases when the formaldehyde had stood in contact with the milk, and hence it is fair to presume that in so far as the enzymic action in the intestinal canal is concerned, transforming solid food into soluble materials which may enter the circulation, there is evidently a stimulating effect produced.

There are, however, many varying conditions which must be considered in properly interpreting the data. The uniformly increased absorption of the proteid elements of the food, and also of

the sulphur and phosphoric acid, accompanied in the first two instances by a decrease in the metabolized elements excreted and in the last instance, namely, phosphoric acid, by a pronounced increase in metabolism, makes the explanation of the data rather difficult. Attention should be called to the fact that while the variations from normal metabolism are not very wide, the individual data are remarkably uniform and consistent.

The conditions which are noted in the case of the proteins would lead one to expect a gain in the body weight. This expectation, however, is not realized for either class of subjects, although the losses in weight are so slight as to be practically negligible. The ratio of the food weight to the body weight was uniformly maintained throughout the experiment, and, hence, if no variations in metabolic activity had occurred a fair presumption would have been that the body weight would remain constant. That the change of weight was slight in the view of the disturbances of the metabolic functions may be accounted for by the inhibiting or retarding influence of the preservative upon the nitrogen and sulphur katabolism, or by the slight increase in water in the urine and feces. It cannot be maintained, however, that a retarded katabolism is beneficial to health. On the contrary a more rapid renewal of the tissues within the limits of healthy activity would be more likely to preserve a normal condition. The old tissues cannot be expected to functionate as perfectly as those which are newer, and hence, within reasonable limits, a change of the tissues of the body must be considered as necessary to a healthy condition, and the maintenance of a normal vitality.

The medical data indicate plainly that formaldehyde, even when given in small quantities, is an irritating substance to the mucous membrane, and, therefore, the normal organs are at first actively stimulated to rid themselves of the irritating foreign substance. It is not strange, therefore, that this preservative had a marked stimulative action on those organs and cells secreting the various digestive juices. It is evident that when the digestive and excretory organs of the body are excited to unusual activity by such an extraneous body having neither food nor condimental value, they act in self defence, and it would be wholly illogical to conclude from

this increased excitation that these bodies were helpful to digestion and conducive to health. The nature of the investigation made it impossible to determine whether any organic change took place in the various organs affected, but it may be assumed that any such change which these organs had undergone in the limited time was not sufficient to disturb in any notable way their normal functions which they would perform until the continued administration of the drug produced disease due to the excessive stimulation.

In the case of phosphoric acid, the increased katabolic activity is difficult of definite interpretation, though it is established beyond doubt that such an effect is produced. The formaldehyde may exert a selective action for those proteid bodies high in phosphorus, rendering them insoluble, but in this case there would be an excess of phosphorus in the feces, which is not found. Or the formaldehyde may induce a change in the process of digestion whereby the phosphorus of the food is changed into a soluble and easily excreted form without passing through the tissues of the body. This might easily be the case if in the process of digestion the glycerol-phosphoric acid formed is transformed into soluble inorganic salts, which are readily excreted. Whatever may be the explanation, the changes indicated in normal metabolism, accompanied as they are by the development of the symptoms described, can only be considered as prejudicial to health.

The general tendency to produce a slight decrease in the temperature of the body, assuming for the moment that the data warrant the conclusion that such a condition of affairs existed, might well be due to the inhibition of cell activity shown by the retardation in the breaking down of tissues. The normal functions of the body would doubtless be disturbed by such a condition, aside from the irritating and other disturbing influences exerted by the exhibited drug.

The tendency of the preservative to produce albumin in the urine, while not well marked, is at least worthy of attention. The fact that only slight changes take place in the body weight is sufficiently explained in the data and cannot be urged in favor of the exhibited preservative.

Apart from the injurious effects of formaldehyde itself, its use

as a food preservative would be especially inadvisable in milk or cream, because its addition in dilute solution prevents the growth of acid-forming bacteria, but has no effect in retarding the action of many harmful organisms; in other words, the milk is prevented from becoming sour and thus indicating its age and the danger signal is thus removed, while the other organisms which are capable of producing disease continue to multiply in the milk with practically the same degree of rapidity as if the formaldehyde was not present.

The final conclusion, therefore, is that the addition of formaldehyde to foods tends to derange metabolism, disturb the normal functions, produce irritation and undue stimulation of the secretory activities, and, therefore, it is never justifiable.

Sulphate of Copper.—The data which have been collected in the course of this experiment have led to the conclusion that the administration of sulphate of copper even in the extremely small quantities in which it has been given has a very distinctly unfavorable effect upon health and digestion, as indicated by the ordinary clinical and medical summaries. Severe pains are produced in the stomach accompanied often with nausea and sometimes with vomiting, there is a general tendency to malaise, often a development of headache, and other unfavorable symptoms of a more or less persistent and uniform character. Further than this, the symptoms which are usually not developed for about a week continue in some instances for a number of days into the after-period after the sulphate of copper has been withdrawn. The data indicate that copper, like many other metals, is likely to produce a cumulative effect, and that its administration in even much smaller quantities than those indicated, or less than those which would be ingested in the regular consumption of coppered vegetables, is attended with more or less danger on this account.

There was a very small loss of weight in nine of the subjects, while the three who showed the greatest tolerance of the copper sulphate gained in weight. No definite conclusions can, therefore, be formed respecting the general effect upon the weight of the body, except that in the cases where uniform effects are produced there is a slight loss of weight.

The copper salt which was used in this experiment differs from

other chemicals which have been used in this series of investigations in that its excretion falls only partly upon the kidneys. The effect produced on the urine, therefore, cannot be ascribed directly to the copper salt employed, but only to such derangements of the metabolism due thereto as would incidentally affect the composition of the urine.

The effect upon the general metabolism is of a character which, though not very pronounced, is indicative of a retardation of normal metabolic processes. Inasmuch as a small quantity of sulphur was introduced into the system through the copper salt, the quantity of this sulphur must be taken into consideration in studying the effect on metabolism. There is seen to be quite a uniform tendency to derange the ratio of the metabolized sulphur and nitrogen.

The apparent increase in the relative quantities of sulphur excreted is due rather to the diminution in the nitrogen than to an actual increase in the sulphur over that which would be expected from the ingestion of the sulphuric acid in the copper salt. The most marked change in the sulphur compounds is in the case of neutral sulphur, which shows a decided and uniform increase during the administration of the copper salt and in some cases for several days thereafter.

The effect produced upon the metabolism of nitrogen is more important. Under the administration of sulphate of copper there is a marked and constant decrease in the excretion of urea, which is a matter of great significance. Such a decrease can only be regarded as an indication of a retarding effect on nitrogen metabolism. At the same time the quantity of uric acid and xanthin bases are increased during the administration of the copper salt and the increase in xanthin is still very marked in the after-period. These two important observations indicate that the nitrogen metabolism is disturbed in a way which must be considered injurious to health.

There is also a notable effect produced upon the phosphoric acid metabolism. There is a marked decrease in the total metabolized phosphorus, and, while the non-metabolized phosphorus is less uniformly affected, there is a decided tendency shown to decrease the

total excretion of phosphoric acid under the influence of the copper sulphate.

The final conclusion, based on the medical and clinical data and on the study of the effect of the copper sulphate upon metabolism, is that the administration of this salt is prejudicial to health.

Potassium Nitrate.—It is evident that the administration of small quantities of potassium nitrate induce only slight disturbances in the metabolic processes, and indicate only to a slight degree harmful or deleterious effects as noted in the medical and clinical data. It is evident moreover that with the exception of one instance, namely, the increase of the number of red corpuscles in the blood, that no beneficial effect can possibly be attributed to the exhibition of this chemical.

While the data are in this case far less conclusive than those in any of the preceding cases, they are of a character to warrant the suggestion that so far as health and digestion are concerned it is safer to omit a body of this kind from the food. There are some foods which naturally contain small quantities of potassium nitrate. Its very poisonous action when taken in large doses, however, is a warning which should cause great care in its use even in small quantities and deter any one charged with the protection of the public health from expressing any favorable opinion in respect to its use.

It is evident that potassium nitrate in the quantities used has neither a preserving effect nor has it any condimental value. Whatever may be said to the contrary, it is perfectly evident that the sole purpose of its use is the intensification of the red color of meats after preservation. Whatever may be the ethical principle underlying this use of potassium nitrate is a question which is not the subject of discussion in a bulletin of this kind, but it is only due to the consumer that the real purpose of using potassium nitrate in the curing of meats should be revealed.

The further question arises as to whether or not the coloring of preserved meats in this way in order that they may have the color of fresh meats is a violation of the Food and Drugs Act, which forbids the coloring of food products for the purpose of concealing damage or inferiority.

While, therefore, the data which have been accumulated are not such as to warrant a sweeping condemnation of potassium nitrate in foods, they are sufficiently indicative to justify the conclusion that its presence in foods is undesirable and open to suspicion.

General Considerations.

Having thus set forth the general results of this long and laborious study, it is seen that if the conclusions based upon the experimental data are correct that there can be no justification of the process of adding chemical preservatives to human foods. Successful manufacturing establishments have demonstrated beyond peradventure that better, more wholesome, and more permanent forms of food products can be produced without the aid of any preservative whatever. Sterilization will preserve sweet cider better than benzoate of soda. Proper care in handling fruits and in conducting the manufacturing processes for preserves, jams and marmalades will make a more palatable product and one that keeps better than the use of salicylic acid. Careful curing of meats and proper care in transportation will preserve these meats better than boric acid. The natural color of the pea kept in a sanitary can where its color is not lost by action due to imperfections of the tin will make a far more palatable article than will the use of sulphate of copper, and so on to the end of the list. There is no single food product which is not more palatable and of equal if not better keeping qualities when made carefully without the use of preservatives. There is, therefore, absolutely no commercial necessity for the use of these bodies, but it is urged by those who employ them that even though considerable quantities of these bodies are injurious to the health, which no one denies, yet in the minute quantities in which they are used in foods they can not be regarded as in any way deleterious. It is easy to show that such an opinion is without scientific basis. It is quite impossible for any expert who holds this opinion to indicate to any jury, much more to the great jury of the American people any point in the addition of the preservative to food at which it remains harmless, or the point at which it begins to be harmful. Unless such a point could be fixed and demonstrated upon reliable

experimental data, it is evident that no scientific reason can be urged for the use of limited quantities of a preservative, which is acknowledged to be harmful, on the ground that in such quantities it is not injurious.

Inasmuch as a preservative is not a food, and as it does not in any way take part in the nourishment of the body nor in the restoration of waste or growth; and further as it is necessarily eliminated, either unchanged or in other forms which may be even more harmful than the original, by the excretory organs of the body, thus imposing upon them an unnecessary and injurious burden and affecting more or less the constitution of the ultimate cells thereof in an unfavorable way, it is evident that the argument which would permit their use in small quantities is wholly illegitimate.

The fallacy of the argument that small quantities of an injurious substance are not injurious may perhaps be best represented graphically. The chart which accompanies this discussion shows theoretically the normal and lethal dose of a food and a drug or, as in this case, a chemical preservative. The chart shows two curves, one representing a chemical preservative and one representing a food. The normal dose of a food is that quantity of food which maintains a healthy adult body in equilibrium. It is represented on the right of the chart by the number 100. If the quantity of food necessary to maintain the equilibrium in a healthy adult body is slightly diminished, no apparent change is at first experienced and possibly even no discomfort. If, however, the quantity of food be still further diminished progressively, as indicated by following the curve down to the left, the point is finally reached when no food is given at all and death ensues, represented by zero on the left hand of the diagram designated "lethal dose." As the curve begins to deviate from the perpendicular on the right the degree of injury is very readily noticed and starvation or symptoms of starvation are set up. Thus, if you follow the perpendicular on the right downward to the point 80, the divergence of the corresponding point of the curve is already measurable. As you descend to zero the magnitude of the measurement increases. It requires but very little further illustration to show how easily the effect of diminishing the normal dose of

a food can be measured immediately after the curve begins to vary appreciably from the perpendicular on the right.

Let us now consider the perpendicular on the left, which is marked at the top under the term "lethal dose," viz.; a quantity of the added preservative sufficient to destroy life. The normal dose of such an added chemical preservative is 0, and is shown at the base line to the right marked "normal dose." If you add a very minute quantity of a chemical preservative, the curve representing it varies

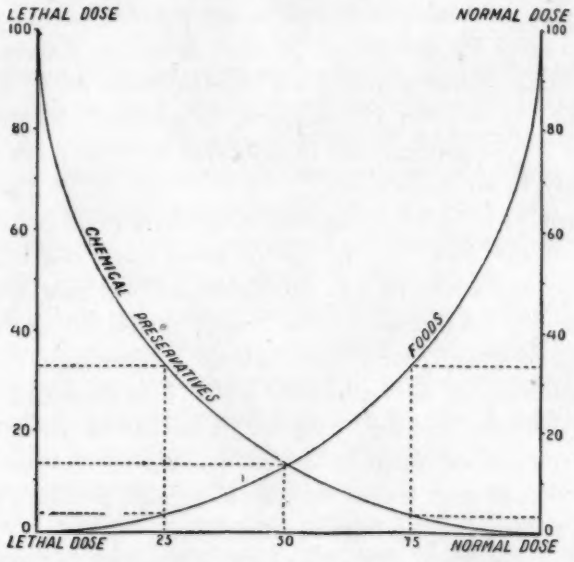

FIG. 1. Graphic chart representing the comparative influences of foods and preservatives.

so slightly from the horizontal base as to be impossible of measurement by ordinary means. If we follow along to the number 75, on the horizontal base, we see the deviation of the curve is sufficiently great to measure. At 50 it is still greater, at 25 still greater, while at the left of the basic line it is a maximum, extending from 0 to 100, or the lethal dose. It is easy to show by mathematical data that no matter how small the quantity of an injurious substance or preservative is, it will still produce an injurious effect, which may be infinitely small if the dose be infinitely small. It follows then, as a

mathematical demonstration, that any quantity of an injurious substance added to a food product must of necessity be injurious, provided it is in the nature of a drug and the body is in a perfectly healthy normal condition.

Hence the argument which has been so persistently urged in favor of a chemical preservative that if in small quantities it is harmless is shown to be wholly untenable. Where there is no necessity for the addition of a harmful substance, where no particular benefit is secured thereby, and where there is no disturbance of the normal state of health there can be no possible excuse of a valid nature to offer for the exhibition of even minute quantities. That these minute quantities would not be dangerous, in so far as producing any fatal effect is concerned, is conceded, but that, in the end, they do not produce any injury, even in these small quantities, is certainly to be denied.

The course of safety, therefore, in all these cases is to guard the opening of the door. If the use of small quantities is permitted, then there can never be any agreement among experts or others respecting the magnitude of the "small quantity," and continued litigation and disagreement must follow. On the other hand, when the harmfulness of any substance which it is proposed to add to food is established and no reason for its use can be given other than the convenience, carelessness, or indifference of the manufacturer, the exclusion of such bodies entirely from food products follows as a logical sequence and a hygienic necessity.

THE HUMMING TELEPHONE,

A Contribution to the Theoretical and Practical Analysis of its Behavior.

By A. E. KENNELLY and WALTER L. UPSON.

(*Received July 20, 1908.*)

The following paper describes the salient features of an experimental research on the humming telephone, conducted in the Graduate School of Applied Science of Harvard[1] University during the year 1907–08, and discusses an elementary mathematical theory which the observations appear to indicate and support.

Definition.—A "humming telephone" is a connection of:

1. A telephone receiver, or ordinary hand 'phone.

2. A telephone transmitter, or ordinary carbon microphone.

3. A source of electric power, such as a voltaic battery and telephone induction coil, with the receiver in such electric and acoustic relation to the transmitter, that it is able to emit a sustained note or hum. This auto-excited hum may be so loud as to be heard in a distant room through several partitions.

Historical Outline.—The fact that a telephone receiver held, either in contact with, or close to, the face of its transmitter may cause the production of a hum or singing tone, appears to have been first observed by Mr. A. S. Hibbard.[2] This experimental fact is now well known to telephonists. In many cases, it is only necessary to lift a subscriber's telephone from its hook, and hold it face to face with its transmitter, in order to produce a loud hum.

The only published investigation of the humming telephone that the authors have succeeded in finding is an important paper by Mr.

[1] "Investigation of the Phenomena of 'The Humming Telephone,'" by Walter L. Upson, a thesis towards the degree of master of science in electrical engineering, Harvard University, 1908.

[2] September, 1890. See Gill's paper hereafter referred to.

F. Gill,[3] read before a meeting of the Dublin Local Section of the Institution of Electrical Engineers in April, 1901. Very briefly, the salient experimental facts reported in this valuable paper are:

1. The reversal of the telephone receiver connections in the circuit alters the pitch of the auto-excited tone, the pitch being higher for one direction, and lower for the other direction, of connection.

2. The pitch of the tone may also be altered by changing: (*a*) the inductance, capacity or resistance of the circuit, or circuits; (*b*) the strength of current in the microphone transmitter; (*c*) the distance between the receiver and transmitter diaphragms; (*d*) pressure on either of the diaphragms.

The Gill paper does not discuss the theory of the subject beyond suggesting that the phase retardation of the acoustic impulses reaching the transmitter from the receiver has a controlling influence on the pitch of the tone.

The research reported in this paper may be regarded as extending the investigation from the stage reached in Gill's paper to a stage which admits of a first approximation theory. A large amount of research remains, however, to be carried on in the future, before the experimental and theoretical analysis of this fascinating but complex phenomenon can be regarded as satisfactorily nearly complete.

Method of Observation Employed.—As pointed out in Gill's paper, the pitch of the note emitted by the humming telephone, although substantially constant under fixed conditions, is affected by almost any change in the apparatus, in a seemingly most intricate manner. In order, therefore, to study the effect of varying one particular variable at a time, the device was hit upon of acoustically connecting the receiver and transmitter diaphragms in a definitely controllable way by means of telescoping tubes fitting on to the receiver and transmitter faces. These tubes, and also the standard electric connections employed, are indicated in Fig. 1.

The transmitter was kept stationary, with one end of the tube covering and secured to its cone. The receiver was fastened, on a sliding wooden carriage, to the other end of the telescoping tube.

[3] "Note on a Humming Telephone," by F. Gill, *Journal of the Institution of Electrical Engineers*, 1901–02, Vol. XXXI., No. 153, pp. 388–399.

The distance between the faces of the two instruments could be varied at will by pulling out, or pushing in, the telescoping tube-sections. The average current in the primary circuit was measured with a Weston d.c. milliammeter. The pitch of the humming note was measured approximately by the ear, with the aid of a number of short organ pipes, and, in some instances, with the aid of a violin. The voltaic battery used consisted of a selected number (from two to nine, but usually four) of 25-ampere-hour lead storage cells. The reversing switch in the secondary circuit enabled the receiver terminals to be reversed at will.

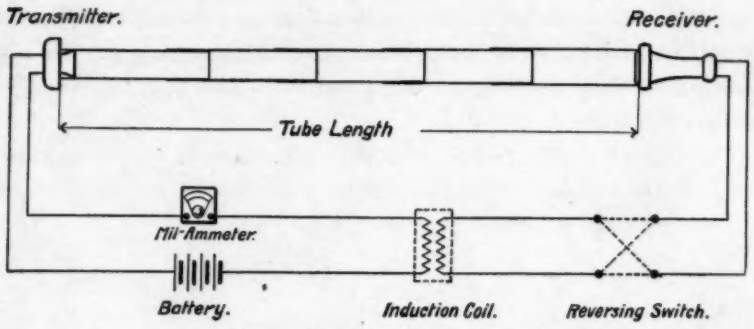

FIG. 1. Diagram of Humming Telephone Connections.

The Telescoping Tubes.—The tubes were made of heavy wrapping paper. Their internal diameters varied from 5 cm. (2 in.) to 6 cm. ($2\frac{1}{3}$ in.). They were used in lengths of 65 cm. ($25\frac{1}{2}$ in.), with a few shorter and longer sections for special measurements. The substance of which the tubes was composed did not appreciably affect the observations. It was found, however, that if the telescoping sections did not fit fairly tightly, erratic results were obtained. Closely fitting sections were used.

The Transmitters.—The transmitters used were of the standard Western Electric Co. type and manufacture. The diaphragm in these instruments was of aluminum, 6.32 cm. (2.49 in.) in total diameter, and 0.55 mm. (0.022 in.) thick, over a coating of Japan varnish on one face. The diaphragm was loaded at its center with one of the disk electrodes of the carbon microphone. The diaphragm was damped by being clamped between rubber rings to an

internal diameter of 4.8 cm. (1.9 in.), and also by the application of a pair of rubber-tipped flat metal springs to areas between the center and edge. The resistance of the microphone varied between the approximate limits of 20 ohms when quiescent, and 110 ohms when in powerful vibration.

The Receivers.—The receivers used in most of the measurements were of the standard bipolar Western Electric Co.'s type, known as No. 122, having poles 1.4 × 0.2 cm. (0.55 × 0.08 in.), separated by 0.82 cm. (0.325 in.). They had a resistance of 210 ohms, and an inductance of 0.025 henry, at a frequency of 1,000 ~. With steady currents, their resistance, at 15° C., was about 70 ohms. The diaphragm of varnished ferrotype iron had an external diameter of 5.5 cm. (2.17 in.), a clamping diameter of 4.95 cm. (1.95 in.) and a thickness, over varnish, of 0.292 mm. (0.0115 in.). Its weight was 4.0 grammes.

The Induction Coil.—The induction coil used was of the standard Western Electric Co.'s type, known as No. 13. Its resistances and inductances were taken as follows:[4]

TABLE I.

Frequency Cycles per Second.	Resistance at 18° C.		Self-Inductance.		Mutual Inductance, Henrys.
	Primary Ohms.	Secondary Ohms.	Primary Henrys.	Secondary Henrys.	
0	1.62	20.3			
1,000	3.2	48.1	0.0044	0.0765	0.0172

The principal dimensions of the coil were: Length over all 8.2 cm. (3.16 in.). Interflange 6.3 cm. (2.5 in.). Diameter over outside cover 2.5 cm. (1 in.). Internal diameter of core tube 0.75 cm. (0.296 in.). Diameter of iron wires in core 0.0356 cm. (0.014 in.). Total number of iron wires in core about 75.

Observation Series No 1. Effect of Shortening the Tube.—Commencing with the connections of Fig. 1, a battery of 8.6 volts, and a tube length of 267 cm., as indicated in Fig. 2 on the scale of abscissas, a loud steady note between G″♯ and A″ (850 ~) was sustained in the telephone. The pitch of this note is shown at *P* on the

[4] The data for the coil at 1,000 ~ were kindly supplied by the engineering department of the Western Electric Co.

upper ziz-zag line *I*. The current strength, on the d.c. milliammeter, as shown at *p* on the lower ziz-zag line *I*, was 130 milliamperes. When the telescopic tube was gradually shortened, the pitch of the note steadily rose, until it reached *Q*, *at* A″♯ (920∼), with 240 cm. of tube-length, and a primary current strength *q* of 200 mas. The intensity of the note near 920 ∼ was ordinarily somewhat weaker than when near 825 ∼. On continuing to shorten the tube, the pitch suddenly broke from *Q*, at 920 ∼, to *R* at 825 ∼. Pushing

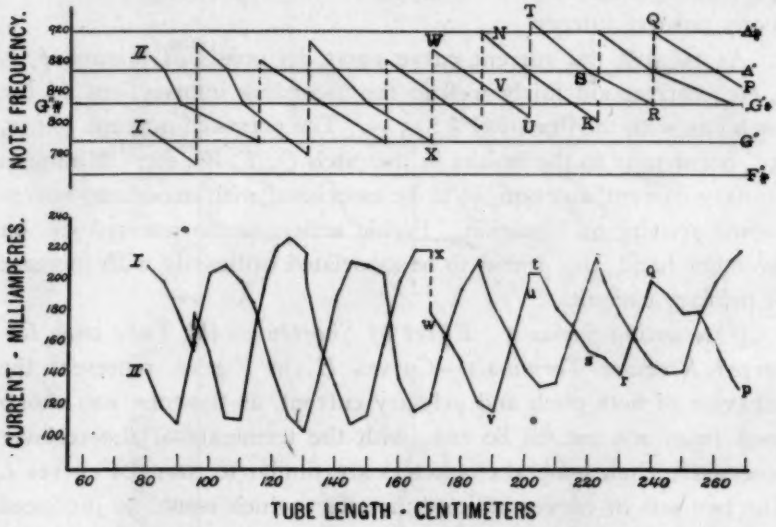

FIG. 2. Effect of Shortening Tube, and of Reversing Receiver Connections.

in the tube further, the pitch would again climb steadily to *T*, at 201 cm., with a new maximum of current. Beyond this point, the pitch would break suddenly to *U* at 810 ∼. Again it would climb to *W*, at 170 cm. and suddenly collapse to *X*. Continuing in this manner, the pitch would alternately rise to maxima and break suddenly to minima, along the pitch zig-zag *I*. At the breaks of pitch, the current would sometimes break to a lower value, as at *t*, *u*; or break to an upper value, as at *w*, *x*; or vary suddenly in rate of change, without discontinuity in magnitude, as at *q*. Repeating the experiment, the zig-zag lines of pitch and of current would be repeated, not exactly but substantially, the variations being due not

merely to observational error, but also to variations in the behavior of the transmitter.

The zig-zag pitch line *PQRST* is found to be somewhat irregular. The slants are by no means regularly parallel. The breaks *QTW* are neither regularly elevated, nor regularly spaced. The only substantial regularity is in the spacing along the pitch line G″♯ of 825 ∼. The intersections of the ascending branches with this line lie approximately 40 cm. apart, at 110, 150, 190, 230 and 270 cm., or in accordance with the series $30 + 40m$ cm., where m is any positive integer.

As regards the current curve *pqrst*, its points of minima *p*, *r*, *v*, etc., correspond fairly well to the ascending intersections of the pitch line with the line of G″♯ 825 ∼. The points of maxima *q*, *t*, *x*, etc., occur near to the breaks in the pitch *Q*, *T*, *W*, etc. Minimum primary current was noticed to be associated with maximum microphonic activity of vibration. Feeble action in the microphone, on the other hand, was found to be associated ordinarily with increase of primary current.

Observation Series 2. Effect of Shortening the Tube with Reversed Receiver Terminals.—Curves *II*, in Fig. 2, represent the behavior of note pitch and primary current, as the tube was shortened from 265 cm. to 80 cm., with the terminals of the receiver reversed. Their general characters are similar to those of curves *I*. The two sets of curves indicate the effect which would be produced by reversing the receiver terminals at any particular tube-length within the above range. Thus, at *S*, or 220 cm., a reversal would lower the pitch from 870 ∼ on curve *I* to *K*, at 810 ∼, on curve *II*. On the other hand, a reversal made on curve *I*, at *V*, of 825 ∼, would raise the pitch to *N* of 900 ∼ on curve *II*, so that whether the reversal produces a rise or fall of pitch depends, in general, upon whether the reversal is effected above or below the mean pitch of G″♯, 825 ∼.

The only apparent regularity in the pitch line *II* lies in the spacing of the ascending intersections with the line of mean pitch G″♯ (825 ∼). These occur near to 90, 130, 170, 210 and 250 cm. of tube-length, or according to the series $10 + 40m$ cm. On the mean-pitch line, the ascending intersections of one curve lie ap-

proximately 20 cm. from, or midway between, those of the other curve.

The note frequencies and primary current strengths for tubes of less than 60 cm. in length are given in Fig. 3, commencing at 60 cm. and shortening down to about 1 cm., when the receiver face came into contact with the transmitter face (cone removed), and so prevented closer approach. Curves *I* and *II* of Fig. 3 correspond

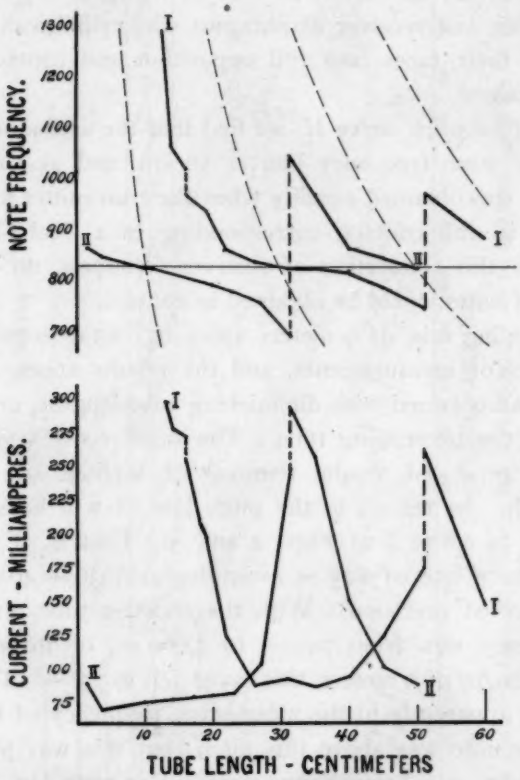

FIG. 3. Humming Note Frequencies and Primary Current Strengths with Short Tubes.

to curves *I* and *II* of Fig. 2, respectively, and indicate the effect of reversing the receiver terminals. It may be observed that following the pitch line *I*, the ascending branch intersects the mean frequency line of 825 ~, at a tube-length of 30 cm., for the last time.

Shortening the tube beyond this point, the pitch rises until it reaches e''' of 1,300 ~, at 12.5 cm., and at a primary current strength of 300 mas. Here the note breaks without descending to a new low note. There is silence with this connection of the receiver between 12.5 cm. and 0 cm. With the transmitter and receiver touching each other, it was possible to produce almost any note between 620 ~ and 1,300 ~, by giving suitable opening to the air at one side. If, however, the outside air was shut off, and the air between the transmitter and receiver diaphragms was cylindrically enclosed, by bringing their faces into full opposition and contact, no note could be obtained.

If we follow pitch curve *II*, we find that the ascending branches intersect the mean-frequency line at 50 cm. and at 10 cm. The pitch 866 ~ was obtained steadily when the transmitter and receiver faces were in full contact, corresponding to a "tube-length" of 1 cm. With this connection of receiver terminals, no other note, or variety of notes, could be obtained at contact.

A telescoping tube of 9 meters (29.5 ft.) total length was used in one series of measurements, and the results appear in Fig. 4. They were all obtained with diminishing tube-lengths, or with compression of the telescoping tube. The small crosses indicate discontinuities produced at the removal of sections of tube when finished with. In regard to the pitch line, it will be seen that it corresponds to curve *I* of Figs. 2 and 3. That is, it crosses the mean-frequency line of 825 ~ ascendingly at $30 + 40m$ cm. with a fair degree of precision. With the shortest tube, the range in pitch-frequency was from 740 ~ to 1,060 ~, or through 320 ~. At the full length of 9 meters, this range fell to 75 ~. The ultimate limit tended apparently to the mean-pitch frequency of G''♯ 825 ~. The average note was above this pitch; but this was probably because the tube was being compressed. Reference to Fig. 5 will show that, when shortening the tube, the average pitch lies above the mean of 825 ~; while in lengthening the tube, the average pitch lies below.

The primary current strength in Fig. 4 tends, in general, to minima at the mean-frequency pitch of 825 ~, and to maxima at the breaks. The differences in current strength become, however,

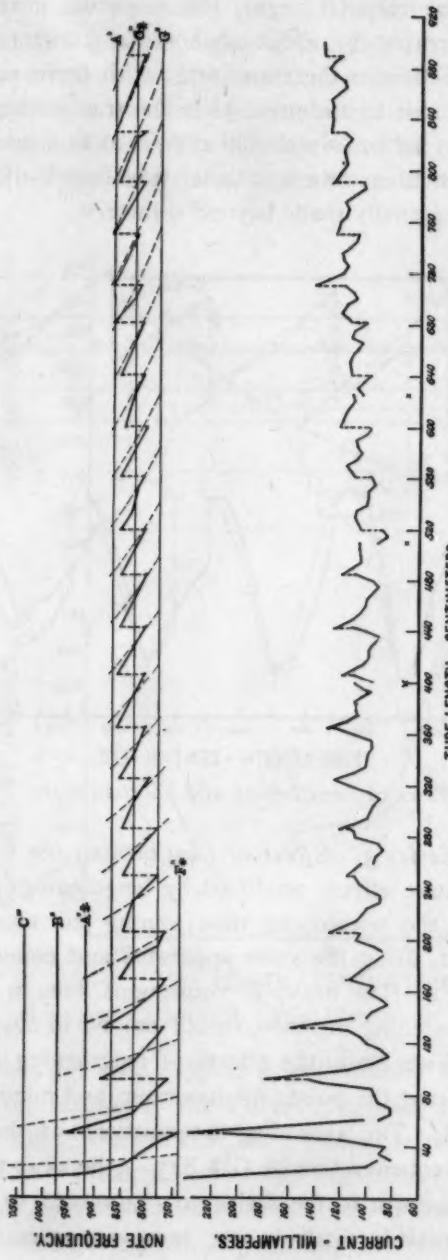

FIG. 4. Frequency and Primary Current Curves for Tube-lengths up to 920 cms. Steadily Reduced in Length, 8.6 Volts in Primary Circuit.

less marked as the tube is longer, the minimum currents rising, as the length increases, by about 40 mas. in 9 meters, indicating steadily reduced action in the transmitter with increasing distance. Since the current rose to 260 mas. when the transmitter diaphragm was entirely out of action, we should expect, at this rate, to be able to sustain the humming note to a total tube-length of 40 meters; but no tests were actually made beyond 9 meters.

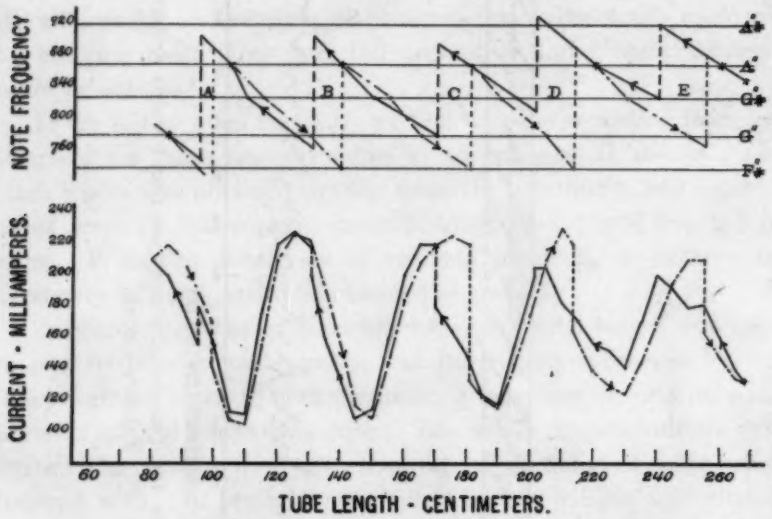

FIG. 5. Effect of Lengthening and Shortening the Tube.

Observation Series 3. Effect of Lengthening the Tube.—Fig. 5 indicates the relative effects produced by lengthening, as compared with shortening, the telescoping tube joining the transmitter and receiver in Fig. 1, using the same apparatus and connections as in Figs. 1, 2, 3 and 4. The heavy or continuous lines in Fig. 5 show the effects of shortening the tube, or correspond to curves *I* in Fig. 2. The broken lines show the effects of lengthening the tube. It will be observed that the points of maximum and minimum current agree fairly well. The ascending intersections of the pitch lines with the mean-frequency line of G″♯ 825 ∼, lie near together, and approximately conform to the series $30 + 40m$ cm. of tube-length. The points of break in pitch do not, however, agree, and the dis-

tances between corresponding pairs of breaks in pitch increase as the tube-length is greater, being 4 cm. at A, 9 at B, 11 at C, 13 at D, and 15 at E. Although not shown in Fig. 5, owing to limitations of space, it was found that these distances between corresponding breaks continued to increase until they reached about 20 cm., after which they shortened again to commence a new expanding series.

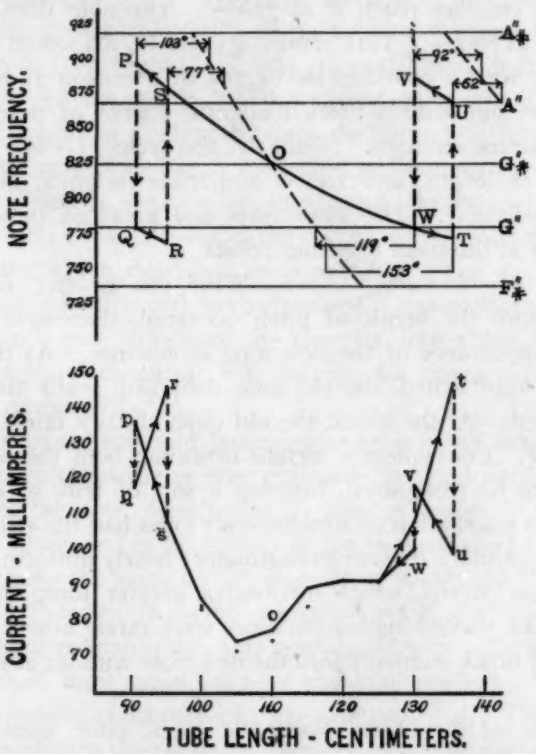

FIG. 6. Humming Cycles with Cyclic Changes in Tube-length.

Observation Series 4. Effect of Alternately Reversing, or Reciprocating, the Motion of the Tube. Humming Cycles.—If, when compressing the telescopic tube, and when the note broke from a higher to a lower pitch, the tube was immediately extended again, the note would continue to lower in pitch for a little while, and then break back to a higher pitch. By moving the tube in and out, like

a concertina, over this range, the pitch would break to and fro in a very regular way. The corresponding reverse action would also occur if the motion commenced with extension. These conditions are shown in Fig. 6. Commencing at the point *O*, with 110 cm. of tube-length, on the mean frequency of 825 ~, if we shorten or compress the tube to 90.5 cm., we reach *P* at 900 ~, near A″♯. The note then breaks to *Q* at 780 ~. Increasing the tube-length back to 95 cm., we reach *R* at 770 ~. The note then breaks upwards to *S* at 880 ~. This humming cycle *PQRS*, could be repeated indefinitely with a considerable degree of precision as to pitch and tube-length; but with a more moderate degree of precision as to primary current strength. Similarly, the cycle *TUVW*, of 10.5 cm. amplitude in length, and 100 ~ amplitude in pitch, might be repeated indefinitely. The amplitudes and areas of these humming cycles vary at different breaking points.

Purity of Humming Tone.—With the greater tube-lengths, shortly before the break of pitch occurred, there was frequently noted an appearance of the new tone in advance. As the breaking point was approached, the old tone dwindled, while the new tone strengthened. At the break, the old tone, already faint, would suddenly cease. Consequently, before breaking, both the old and new tones might be recognized, forming a sort of trill, or combination tone. This association of simultaneous tones had the effect of maintaining the primary current strength more nearly uniform. With the shorter tube-lengths, which involved a greater jump of frequency at the breaks, these combination tones were rarely heard, and the old note would break suddenly into the new note without any suggestion of a trill.

In some of the observations, the notes, aside from the above-mentioned trilling near to the breaking points, gave acoustical evidence of multiple tones. Occasionally, the principal tone was accompanied by an octave overtone. The octave might be either the first octave below, or the first octave above, the principal tone. Such overtones were comparatively faint. At other times, the superposed tone, instead of being harmonic to the principal tone, appeared to differ therefrom by only about one tone on the musical scale. This inharmonic superposed tone was also relatively faint with respect

to the principal tone. Generally, however, no superposed tones could be discerned, and the note was clear and flute-like in quality. Irregularities in the fitting of the telescoping tube-sections, or in other acoustic connections, were found to be productive of superposed notes.

Effects of Electrical Changes.

Observation Series 5. Effect of Resistance in Primary or Secondary Circuit.—In this test a single tube of constant length (86.5 cm. or 34 in.) was used. It was of pasteboard, had an internal diameter of 5.1 cm. (2 in.) and weighed 113.5 gm. This length happens to be about midway between the ascending intersections of pitch lines *I* and *II* in Fig. 2 measured on the mean-frequency line of 825 ~. That is, the tube-length selected favored each of the lines *I* and *II* nearly equally. The battery e.m.f. of 8.6 volts was the same as in all the above described measurements. The same telephone receiver and induction coil were also used. Substantially non-inductive resistance was introduced, by rheostat, into either the primary, or the secondary, circuit at will, leaving the connections of Fig. 1 otherwise unchanged.

After starting the loud humming note with no extra resistance in either circuit, resistance was gradually inserted into the primary circuit until the note, diminishing in amplitude, finally disappeared. The extra resistance in the circuit at the extinction of the tone was recorded, under the name of "*extinguishing resistance.*" Resistance was then withdrawn from the primary circuit, and, after the loud note had been reëstablished, was introduced gradually into the secondary circuit, until again the note was extinguished. The secondary extinguishing resistance was likewise recorded. The same tests were repeated with the telephone receiver terminals reversed.

It was found that both the primary and secondary extinguishing resistances repeated themselves very fairly (within about 5 per cent.) in successive trials. In order to obtain the best comparative results in successive tests, it was found desirable to tap the transmitter gently when approaching the condition of extinction.

The pitch of the tone when enfeebled almost to extinction by extra resistance, in either the primary or secondary circuit, was always close to the mean frequency of 825 ~.

The amount of either the primary or secondary extinguishing resistance was found to depend upon the adjustment and operative condition of the transmitter, keeping the receiver, tube-length and all other conditions unaltered. This led to a trial of this method as a practical test of microphone transmitters.

Observation Series 5a. Test of Transmitter by Hum-extinguishing Resistances.—A number of transmitters, some good and others imperfect, were tested under the conditions above outlined. These transmitters were kindly loaned for this purpose by the Western Electric Co. Twelve were regular standard instruments that had already satisfactorily passed the factory tests. These were labelled T_1 to T_{12} respectively. Four more were marked defective and "down in volume." They were labelled T_{13}, T_{15}, T_{16} and T_{24}. Four more were marked defective and "thick in quality." These were labelled T_{14}, T_{17}, T_{22} and T_{23}. Yet another four were marked defective and "burning." These were labelled T_{18}, T_{19}, T_{20} and T_{21}. Defective transmitters "down in volume" are recognized as weak. Those which are of "thick quality" are strong but defective in articulation. Those which are "burning" produce slight arcing, at or near the electrodes, when subjected to normal conditions of operation.

The results of the tests on these 24 transmitters are given in the accompanying table; where R represents the primary, and r the secondary, extinguishing resistance, when the transmitter was gently tapped. Care was taken that the observer in this test did not know the label number, or reported condition, of the transmitter under trial. It will be seen that with the good transmitters, the mean primary extinguishing resistances were all included between 26.5 and 58.5 ohms, their mean secondary extinguishing resistances being between 1,925 and 4,150 ohms. All of the defective transmitters lay outside these limits, the "down in volume" being low, and the "thick quality" high, in their extinguishing resistances; except two of the "burning" type, which fell within the good secondary extinguishing resistance limits. It would seem, therefore, that this resistance method constitutes a possible practical application of the humming telephone to transmitter testing; except that "burning" transmitters may require a separate test for their detection.

The results recorded in the last two columns of Table II. are presented graphically in the target diagram of Fig. 7. The square includes all the good instruments and none of the bad. The mean of the good transmitters is indicated by the solid black circle.

TABLE II.

Table of Comparative Hum-Extinguishing Resistances for 12 Good and 12 Defective Transmitters.

Quality of Transmitter.	Transmitter.	Extinguishing Resistances.				Average. R.	Average. r.
		1st Position of Rec'r.		2d Position of Rec'r.			
		R (Pri.).	r (Sec.).	R (Pri.).	r (Sec.).		
OK.	T_1	43	3,100	50	2,300	46.5	2,800
	T_2	23	3,050	44	3,000	33.5	3,025
	T_3	55	4,300	43	4,000	49	4,150
	T_4	25	2,300	31	1,900	28	2,100
	T_5	45	3,900	40	2,600	42.5	3,250
	T_6	31	3,000	69	3,800	50	3,400
	T_7	27	2,600	26	1,600	26.5	2,100
	T_8	30	2,600	30	1,700	30	2,150
	T_9	31	2,700	44	2,300	37.5	2,500
	T_{10}	47	3,900	46	3,900	46.5	3,900
	T_{11}	43	4,100	74	3,800	58.5	3,950
	T_{12}	33	2,600	21	1,250	27	1,925
	Mean	36.1	3,180	43.2	2,696	39.6	2,937
Down in Volume.	T_{13}	10	750	10	700	10	725
	T_{15}	6	390	—	—	6	390
	T_{16}	6	900	12	1,000	9	950
	T_{24}	17	1,400	20	1,500	18.5	1,450
	Mean	9.75	860	14	1,070	10.9	879
Thick Quality.	T_{14}	62	4,900	66	2,300	64	3,600
	T_{17}	50	9,000	70	10,000	60	9,500
	T_{22}	52	7,000	75	8,000	63.5	7,500
	T_{23}	66	5,300	78	6,700	72	6,000
	Mean	57.5	6,550	72.5	6,750	64.75	6,650
Burning.	T_{18}	54	3,800	68	3,400	61	3,600
	T_{19}	61	4,700	93	6,900	77	5,800
	T_{20}	82	5,300	102	5,000	92	5,150
	T_{21}	60	3,900	61	2,900	60.5	3,400
	Mean	64.25	4,425	81	4,550	72.6	4,487

Observation Series 6. Effect of Varying the E.M.F. in the Primary Circuit.—Among so many variables and variations as are displayed in preceding diagrams, it is comforting to find one variable which produced relatively little effect within certain practical limits. Fig. 8 shows the frequencies and primary currents for tube-lengths

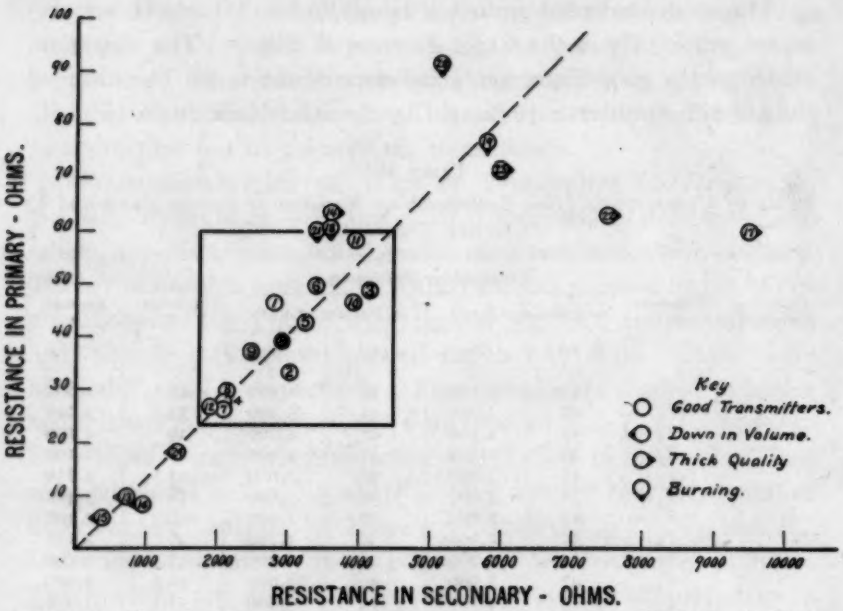

FIG. 7. Target Diagram of Transmitter Tests by the Method of Hum Extinguishing Extra Resistance.

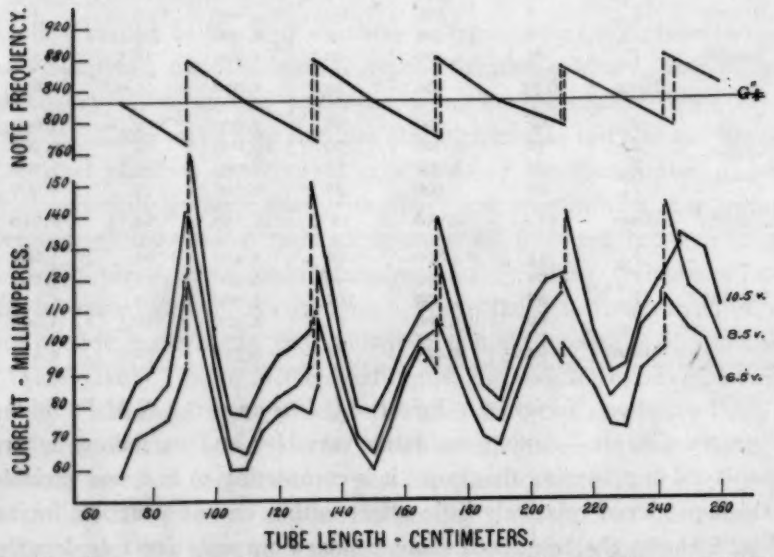

FIG. 8. Frequencies and Primary Currents for Different Primary E.M.F's.

steadily reduced from 260 to 70 cm., with batteries of 3, 4 and 5 storage cells, respectively, in the primary circuit (6.5, 8.5 and 10.5 volts). The transmitter, induction coil, receiver and transmitter were all as in Figs. 1 to 6. It will be seen that the primary currents have their respective maxima and minima in substantial agreement, the range of variation being naturally greatest for the largest battery, and least for the smallest. The ascending intersections of the frequency line with the mean-frequency line of 825 ~ are the same throughout, and conform to the series $30 + 40m$ cm., in agreement with line *I* of Fig. 2. The breaks in pitch do not all coincide; but the differences in this respect are not great, nor can it be said

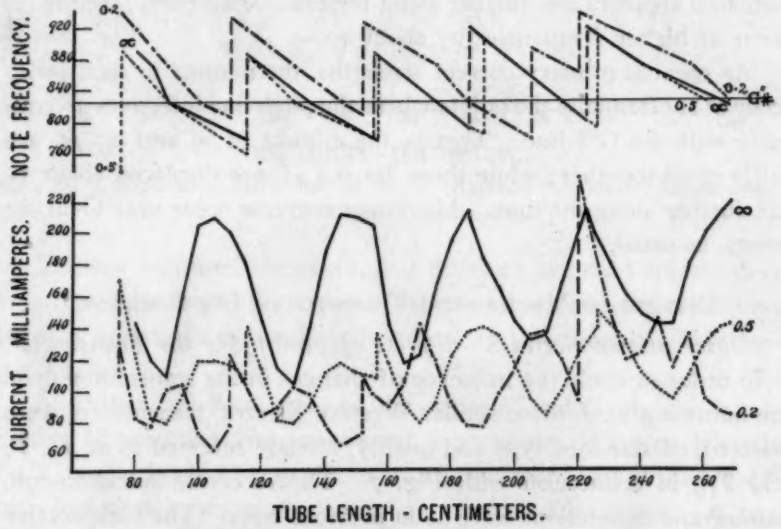

FIG. 9. Frequencies and Primary Current Strengths for Different Condensers in Secondary Circuit.

that the biggest battery always produced the most retarded break. Moreover, excepting perhaps the break at 240 cm., the variations in breaking points are within the limits of variation obtained in successive series with one and the same battery.

Observation Series 7. Effect of a Condenser in the Secondary Circuit.—It was found that a certain magnitude of condenser capacity inserted in series in the secondary circuit had a marked effect on the behavior of the humming telephone. The results are

indicated in Fig. 9, for a tube-length commencing at 270 cm. and steadily reduced to 75 cm., with 8.6 volts in the primary circuit and the same instruments as before. Three sets of curves are given, for 0.2 μf. (microfarad), 0.5 μf., and ∞ μf. (condenser short-circuited), respectively. Referring to the pitch lines, it will be seen that there is not much difference between the cases of ∞ and 0.5 μf. The ascending branches of the zig-zags cut the mean frequency line of G″♯ at 90, 132.5, 175 and 210 cm. or fairly in conformity with the series $10 + 40m$, as in curve *II* of Fig. 2. With 0.2 μf., however, the intersections with this line are at 100, 145 and 185 cm., or more nearly in conformity with the series $22 + 40m$ cm.; that is, at points displaced about 12 cm. further along the tube. Moreover, the breaks occur at higher frequencies by about 40 $\sim$.

As regards primary current strengths, the minima in each series occur at substantially the points where the pitch line intersects ascendingly with the G″♯ line. That is, the minima of ∞ and 0.5 μf. are fairly close together; while those for 0.2 μf. are displaced about 12 cm. further along the tube. Maximum currents occur near breaking points, as usual.

Effects of Mechanical Changes in Instruments.

Observational Series 8. Effects of Modifying the Transmitter.—In order to study the influence of changes in the transmitter upon the humming note, three similar Western Electric transmitters were selected, of standard type and quality, already referred to as T_5, T_8 and T_{11}, in connection with Fig. 7. The receiver, induction-coil, battery and connections were as in previous tests. The comparative results with these three transmitters are shown in Fig. 10, for tube-lengths steadily reduced from 260 to 70 cm. It will be noted that the ascending intersections of the pitch lines all intersect the mean-frequency line of 825 $\sim$ in substantial conformity with the series $30 + 40m$, or in accordance with curve *I* of Fig. 2. The breaking points do not agree, No. 8 always breaking last at a higher pitch, No. 5 next at a medium pitch and No. 11 first at a lower pitch. It may also be noted that in the hum-extinguishing resistance-test of these three transmitters, as given in Table II., and in Fig. 7, their order of succession was the same.

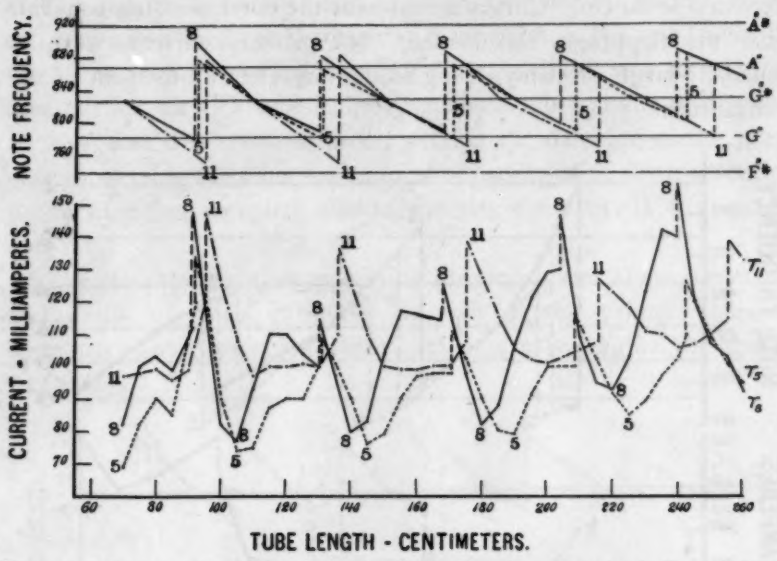

FIG. 10. Comparative Behavior of Three Regular Standard Transmitters with Reduced Tube-lengths.

The test indicates, therefore, that different standard transmitters in normal adjustment do not alter the mean-frequency tube-lengths; but that variations in breaking lengths may be expected within certain limits.

A further test was made of the effect of modifying the transmitter, by selecting for experiment a particular Western Electric Co.'s standard type of transmitter which had been used in the laboratory for some years, and was not in the best adjustment. A test was made with this instrument (using the same receiver, coil, battery and connections as in preceding tests), first without any extra load on its diaphragm, second with a load, and third with the load removed. The load consisted of a small brass disk 1.5 cm. (0.59 in) in diameter, and 0.2 cm. (0.079 in.) thick, clamped at its center between the two small nuts at the center of the external surface of the diaphragm. This added a mass of 2.7 gm. to the vibrating system of the transmitter. The results are seen in Fig. 11. Curves 1 and 3 represent the behavior of the system unloaded, before and after loading respectively, the tube-length being steadily diminished

from 200 to 80 cm. Curves 2 represent the corresponding behavior when the diaphragm was loaded. The primary currents were all unusually large, probably owing to the imperfect adjustment of the transmitter.

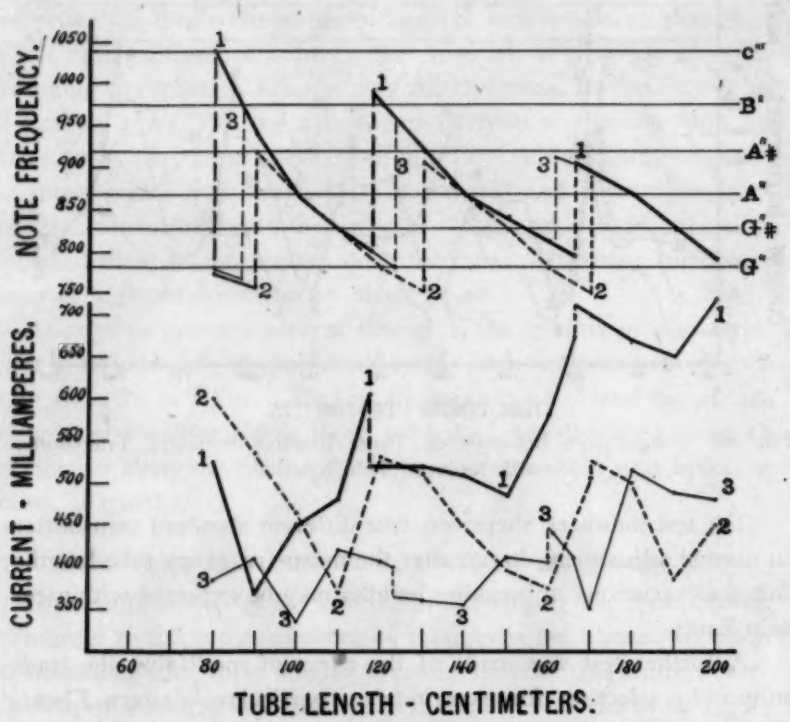

FIG. 11. Test of a Transmitter with its Diaphragm Loaded and Unloaded.

It will be observed that the loading did not appreciably alter the ascending intersections of the pitch lines with the G''# mean-frequency line, which occur in conformity with the series $20 + 40m$ cm. The loading seems to have somewhat lowered the range of pitch as a whole; or to have modified the conditions at breaking, without materially affecting the conditions at mean-frequency ($825 \sim$).

A number of trials with further modifications of the transmitter diaphragm substantiated the above stated results. In one case, a new experimental diaphragm of tinned sheet iron, 0.38 mm. thick

(0.015 in.), with parallel and opposite symmetrical sectors sliced off, was substituted for the regular diaphragm in the test transmitter. The primary current strength during activity was thereby increased; but the G″♯ tube-lengths remained substantially unchanged at $30 + 40m$ cm. Adding loads, altering the damping-spring pressure, or varying the other mechanical adjustments of the transmitter produced either complete silence; or else the usual G″♯, at $30 + 40m$ cm.

The tests showed that modifying the transmitter alters the range and limits of pitch variation, as well as the primary current strengths; but does not sensibly alter the tube-lengths for mean-frequency.

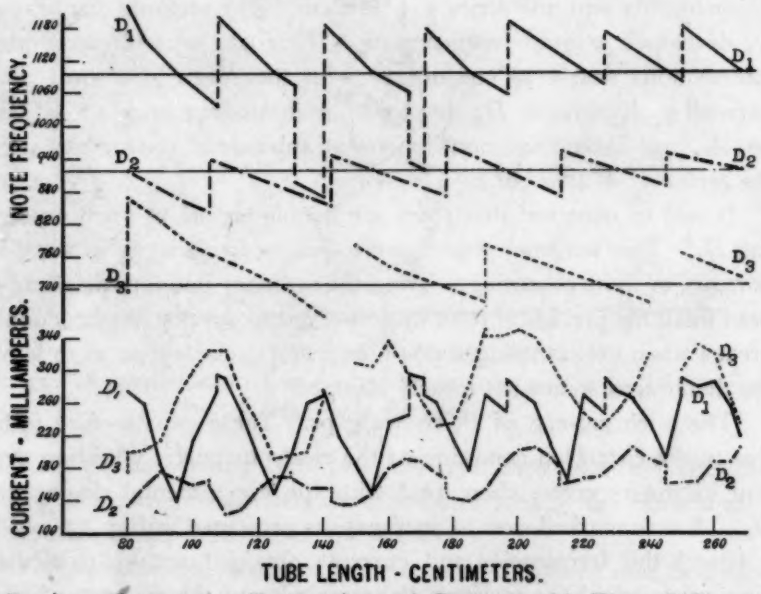

FIG. 12. Comparative Frequencies and Currents with Three Different Receiver Diaphragms.

Observation Series 9. Effect of Altering the Receiver.—In order to determine the influence of the telephone receiver diaphragm on the hum, three special receiver diaphragms were made up, each of soft transformer steel, 0.355 mm. (0.014 in.) thick, and 5.5 cm. (2.16 in.) in diameter, labeled D_1, D_2 and D_3 respectively. D_1 was

left circular, D_2 and D_3 had symmetrical sectors cut from opposite sides, reducing their width to 4 cm. (1.57 in.) and 3 cm. (1.18 in.) respectively. In clamping these strip diaphragms in front of the bipolar magnet of the standard receiver, their angular position did not appear to affect the system appreciably.

The results obtained with these three diaphragms are indicated in Fig. 12, for tube-lengths diminished steadily from 270 to 80 cm. It will be seen that the receiver diaphragm influences the hum profoundly. Thus, the circular diaphragm D_1 developed a mean-frequency of 1,100 $\sim$ or c‴♯, judging by the points of minimum primary current, and its pitch zig-zag formed ascending intersections with this line at 95, 125, 155, 185, 215 and 245 cm., approximately, in conformity with the series $5 + 30m$ cm. The sectored diaphragm D_2 developed a mean-frequency of A″♯, at 920 $\sim$, with ascending intersections nearly in conformity with the series $36m$ cm. The narrowest diaphragm D_3 developed a mean-frequency of F″, at 705 $\sim$, and ascending intersections in substantial conformity with the series $33 + 47m$ cm.

It will be observed that there are double breaks in pitch on zig-zag D_1. This tendency was found to follow irregularity in the diaphragm, or in its mounting. Thus, the ordinary standard diaphragm used in all the preceding tests was observed to develop similar double breaks when the clamping screw-cover was slackened, so as to leave the diaphragm somewhat loosely clamped.

The pitch zig-zag of D_3 shows gaps. These gaps seemed to be due to the enfeebled condition of the electromagnetic vibrating system in the receiver when used with the experimental diaphragm D_3. A very marked case of such gaps is presented in Fig. 13, which indicates the frequencies and currents obtained with a particular single-pole telephone receiver, the remainder of the apparatus being unchanged, and the tube-length being steadily reduced from 165 to 80 cm. The line of mean-frequency is at 1,025 $\sim$, and the ascending intersections with this line are formed at points conforming with the series $28 + 32m$ cm. Only short pieces of the zig-zag were, however, obtainable, and these only with the aid of a condenser in the secondary circuit. The dotted segments RS and TV were obtained with the receiver terminals reversed, and correspond ap-

proximately to ascending intersections of the series $13 + 32m$ cm.

Various other modifications of receiver and receiver diaphragm were tried. Loading the diaphragm with a small central mass lowered the mean humming frequency. By selecting suitable diaphragm dimensions, the mean-frequency of the hum could be varied between wide limits.

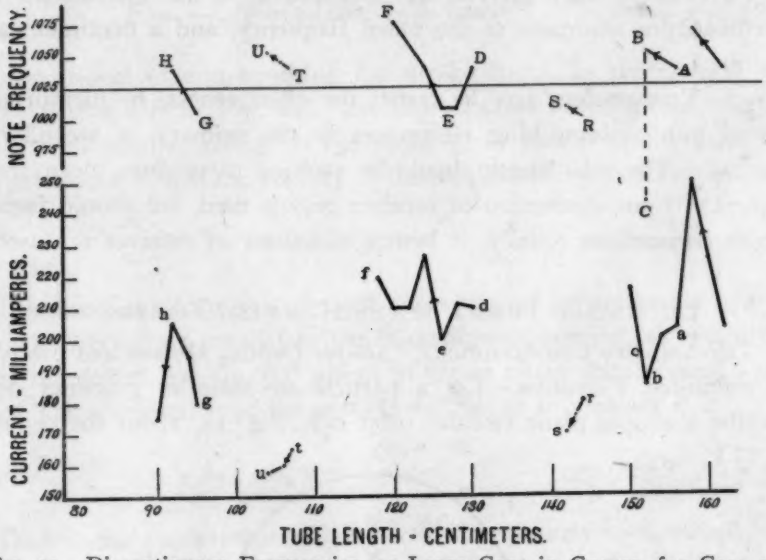

FIG. 13. Discontinuous Frequencies, or Large Gaps in Curves, for Case of Singe-pole Receiver.

Conclusions Directly Derivable from the Experiments.—The following more prominent conclusions are indicated by the experiments themselves, independently of any theory:

1. The mean-frequency of the humming-telephone note is determined solely by the receiver diaphragm, and its natural free rate of vibration.

2. The ascending intersections of the frequency zig-zag with the mean-frequency line will be formed approximately at tube-lengths of $(\frac{3}{4} + m)\ v/n_0$ cm. for one connection, and of $(\frac{1}{4} + m)\ v/n_0$ cm. for the other connection, of the receiver; where v is the velocity of sound in air (33,000 cm. per sec. nearly), n_0 is the mean frequency in cycles per second, and m is any positive integer, within

the working range of the tube. The constants $\frac{3}{4}$ and $\frac{1}{4}$ may be modified by the presence of condensers, and other circumstances.

3. The range of pitch variation, and the breaking positions, are determined by the transmitter, and by the reinforcing capability of the system. For systems that are weak, either electrically or acoustically, the range of pitch, above or below the mean, will be small.

4. The primary current, as measured by a d.c. instrument, is ordinarily a minimum at the mean frequency, and a maximum at a break.

5. Transmitters may be tested for effectiveness, by measuring their hum-extinguishing resistances in the primary or secondary circuit. The tube-length should be such as to produce mean frequency if one connection of receiver only is used, but should favor both connections equally, if both connections of receiver are used.

Outline of Theory of the Humming Telephone.

Preliminary Considerations. Simple Orbital Motion and Simple Unretarded Vibration.—Let a particle of mass m grammes describe a simple plane circular orbit zab, Fig. 14, about the center

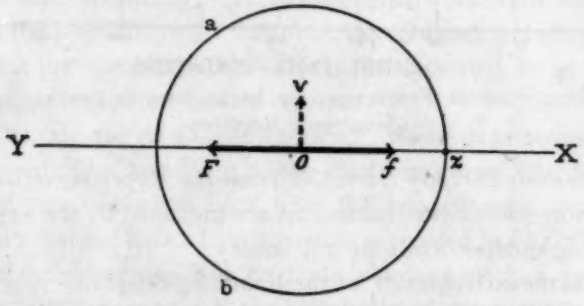

Fig. 14. Vector Diagram of Free Undamped Vibration.

O. Let the radius $Oz = r$ cm., and let OX be the initial line of reference. At time $t = 0$ seconds, let the particle occupy the position z; so that its initial radius vector is Oz. Let ω be the uniform angular velocity of the particle about the center O, in radians per second. Then, after the lapse of t seconds, the particle will occupy a point in the plane defined by the vector displacement

$$\xi = re^{j\omega t} \qquad \text{cms. } \angle \qquad (1)$$

where $j = \sqrt{-1}$, and ξ is the displacement of the particle in cms. from O at the angle ωt, measured positively, or counter-clockwise, from the initial line OX.

Let the particle be acted upon by a centrally directed elastic force

$$F = -A\xi = -ma\xi = -mare^{j\omega t} \qquad \text{dynes } \angle \qquad (2)$$

proportional to and opposing the displacement, as represented by the vector OF in Fig. 14. Let there be no other forces except those of inertia, acting on the particle; so that the movement is frictionless. Then the velocity of the particle at any instant t will be

$$v = \dot{\xi} = j\omega re^{j\omega t} \qquad \text{cms./sec. } \angle \qquad (3)$$

The direction of the velocity will, therefore, be perpendicular to the radius vector, or parallel to the instantaneous tangent, as indicated by the dotted line Ov, 90° ahead of Oz in phase displacement.

The acceleration of the particle will be, at any instant t,

$$c = \dot{v} = \ddot{\xi} = -\omega^2 re^{j\omega t} \qquad \text{cms./sec.}^2 \angle \qquad (4)$$

That is, the acceleration will be directed oppositely to the displacement. Thus at time $t = 0$, represented in Fig. 14, the acceleration will be directed along OY. The virtual reactive force of inertia will be

$$f = -mc = -m\ddot{\xi} = m\omega^2 re^{j\omega t} \qquad \text{dynes } \angle \qquad (5)$$

In Fig. 14, this reactive force of inertia is represented by Of.

In order that the circular orbital motion shall be stable, the sum of the forces OF and Of, of elasticity and inertia must be zero; or

$$OF + Of = 0 \qquad \text{dynes } \angle$$

$$\therefore \quad -mare^{j\omega t} + m\omega^2 re^{j\omega t} = 0 \qquad \text{dynes } \angle$$

whence

$$\omega = \sqrt{A/m} = \sqrt{a} \qquad \text{radians/sec.} \qquad (6)$$

If, therefore, the angular velocity of the motion be numerically equal to the square root of elastic force per unit of mass, the orbit will be circular and stable, and Fig. 14 may represent its vector diagram. The particle z rotates about O, at constant radius with uniform angular velocity ω, and the pair of equilibrating forces OF and Of rotate in synchronism with it. The entire system, Fig 14, may be imagined as pivoted about an axis through O perpendicular to the orbital plane, and spun about this pivot with uniform angular velocity ω.

By a well known proposition connecting simple harmonic vibration with circular orbital motion, the displacements in the former are the projections of the displacements in the latter, upon a straight line passing through the center of the system. In other words, to every case of simple circular orbital motion in two dimensions corresponds a case of simple harmonic vibration, its projection in a single dimension. Consequently, at time t, we have for the displacement in the case of simple vibration,

$$\xi = re^{j\omega t} \qquad \text{cms.} \quad (7)$$

measured along the initial line OX by projection. The real part only of ξ is retained, and the imaginary part ignored. Similarly, the vibratory velocity will be

$$v = \dot{\xi} = j\omega re^{j\omega t} \qquad \text{cms./sec.} \quad (8)$$

taking only the real part of the equation, or the projected value along YOX. Again, the vibratory acceleration will be

$$c = -\omega^2 re^{j\omega t} \qquad \text{cms./sec.}^2 \quad (9)$$

retaining only the real or projected part. Similar reasoning applies to the forces of elasticity and inertia. The same equations appear as in the circular orbit case; but only their real, or horizontally projected values, are retained. Consequently, we deduce that the vibration of a particle possessing elasticity and inertia without frictional retardation will be stable and self sustained under the condition

$$\omega = 2\pi n = \sqrt{A/m} = \sqrt{a} \qquad \text{radians/sec.} \quad (10)$$

where n is the frequency of the vibration in cycles per second.

If, for example, the diaphragm of a telephone receiver had simple elasticity and inertia without frictional retardation, such that the elastic intensity $a = 26.87 \times 10^6$ dynes per cm. of displacement and per gramme mass, then any displacement released would be followed by an indefinitely sustained angular velocity

$$\omega = \sqrt{26.87 \times 10^6} = 5{,}184$$

radians per second, corresponding to $n = 825$ cycles per second. If the initial displacement were $r = 0.01$ cm., the corresponding simple circular orbit, Fig. 14, would have a radius of 0.01 cm., an angular velocity of 5,184 radians per second, an orbital velocity of 51.84 cm. per second, and an acceleration of 268,700 cm. per second. If the elastic force A were 1.3435×10^6 dynes per cm. of displacement and the effective mass were 0.05 gm., the elastic force OF would be 13,435 dynes, and the centrifugal force Of 13,435 dynes, the two being equal and in complete opposition.

Case of Free Vibration Damped and Unreinforced. Spiral Orbital Motion.—In the case of the particle moving about a center, let the motion be retarded by a force f', proportional to the velocity, defined by the relation

$$f' = -\Gamma v = -2m\gamma v \qquad \text{dynes} \angle \quad (11)$$

Then the orbital displacement at any time t becomes

$$\xi = re^{(-\gamma+j\omega)t} \qquad \text{cms.} \angle \quad (12)$$

The orbital velocity is

$$v = \dot{\xi} = r(-\gamma + j\omega)e^{(-\gamma+j\omega)t} \qquad \text{cms./sec.} \angle \quad (13)$$

The orbital acceleration is

$$c = \dot{v} = \ddot{\xi} = r(-\gamma + j\omega)^2 e^{(-\gamma+j\omega)t} \qquad \text{cms./sec.}^2 \angle \quad (14)$$

Each of the above equations defines an equiangular spiral, an inwardly directed spiral in which the curve makes a constant direction $-\gamma + j\omega$ with the radius vector.

The vector diagram for this case is indicated in Fig. 15. Let

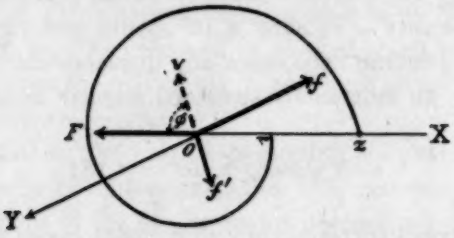

FIG. 15. Vector Diagram of Free Damped Vibration.

z be the position of the particle at any instant. The velocity at this instant will have the vector OV, parallel to the tangent at z, where

$$\tan \phi = \omega/\gamma \qquad (15)$$

The acceleration at the same instant will be directed along OY, the angles XOV and VOY being each equal to the supplement of ϕ. The virtual force of inertia will be directed along Of. The retarding force, opposing the velocity, will be directed along Of'. At any instant the vector sum of the three forces of elasticity, retardation and inertia must be zero. That is,

$$OF + Of' + Of = 0 \qquad \text{dynes } \angle$$

or

$$-mar\epsilon^{(-\gamma+j\omega)t} - 2\gamma mr(-\gamma + j\omega)\epsilon^{(-\gamma+j\omega)t}$$
$$-mr(-\gamma + j\omega)^2\epsilon^{(-\gamma+j\omega)t} = 0 \qquad \text{dynes } \angle$$

whence

$$\omega = \sqrt{a - \gamma^2} = \sqrt{\omega_0^2 - \gamma^2} = \omega_0 \sin \phi \qquad \text{radians/sec.} \quad (16)$$

where ω_0 is the unretarded angular velocity. That is, the angular velocity of orbital rotation has been reduced by the retardation in the ratio of sin ϕ, Fig. 15, and the displacement or radius vector r continually dwindles with time by $\epsilon^{-\gamma t}$.

In the corresponding case of free damped vibration, the above

equations apply; but their real parts only are taken. In Fig. 15, the projections of the vectors on a straight line through O, are selected. The dwindling vibrations of a tuning fork, or the oscillatory discharge of a condenser through a circuit containing resistance and inductance, obey this law. In the last named case, the inductance corresponds to the mass m, the reciprocal of the capacity corresponds to the elastic coefficient A, and the resistance corresponds to the velocity-resisting coefficient Γ. The condenser-charge, or electric quantity, corresponds to the vibratory displacement, the electric current to the vibratory velocity, the discharging electromotive force to the elastic force OF, the resistance e.m.f. to Of', the e.m.f. of self-induction to Of, and the impedance of the discharging circuit to the vector $m\omega_0 \angle\phi$, or $\sqrt{MA} \angle\phi = \Gamma/2 + jm\omega$.

Case of Retarded Free Vibration Reinforced. Restored Circular Orbit.—In order to sustain stable orbital motion in a particle retarded with a force proportional to the velocity, it is necessary

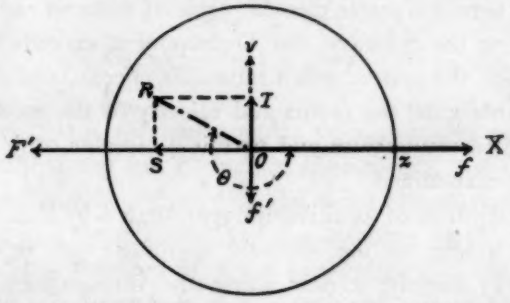

FIG. 16. Vector Diagram of Reinforced Vibration.

to supply energy continuously to the particle and to act upon it with a force equal but opposite to the velocity-resisting force. The orbit will then be restored from an inmoving spiral to a simple circle. The displacement, velocity and acceleration of the particle, Fig. 16, will then be severally expressed by equations (1), (2) and (3) applied to Fig. 14.

Let OR, Fig. 16, be an outwardly directed force from the center O, the magnitude of OR being some function $m\chi(r)$ of the radius of displacement, and θ the phase retardation behind the displace-

ment, reckoned positively in the direction indicated, or clockwise. The restoring force $OR \angle\theta$ may be analysed into two components $OT = OR \sin \theta$, and $OS = OR \cos \theta$ along the directions OV and OX respectively. The component OT may be called the velocity component, or T component, since it acts in the direction of the velocity Ov, and against the retarding force Of'. The component Os may be called the S component, or the new elastic component. It coacts with the elastic force OF that resists displacement.

In order that the circular orbit may be retained, it is necessary and sufficient that the T component of the restoring force shall equilibrate the velocity-resisting force Of'; or, if R be the restoring force, that

$$R \sin \theta + f' = 0 \quad \text{dynes}$$

If the T component should be less than the velocity-resisting force, the system will lose energy. The orbit will spiral inwards until the velocity has been sufficiently diminished to equilibrate the T component, and permit a stable circular orbit of reduced radius to be restored. If, on the contrary, the T component exceeds the velocity-resisting force, the system will accumulate energy, and the orbit will spiral outwards until the radius and velocity of the motion are sufficient to restore equilibrium and permit a circular orbit of enlarged radius to be maintained.

In the condition of equilibrium represented in Fig. 16, we have four forces acting on the particle, forming two separate equilibrating pairs; namely, a pair along the displacement vector Oz, which we may call the displacement pair, and a pair perpendicular thereto, which we may call the velocity pair. Both these pairs rotate together at some uniform angular velocity ω, which will in general differ from that which would hold for unretarded motion ω_0, as in Fig. 14, or from that which would hold for retarded unreinforced motion, as in Fig. 15.

Considering the displacement pair, the first member is the elastic force OF, modified by the new elastic force OS, to OF', Fig. 16. The new virtual force of inertia is Of. Consequently

$$OF' + Of = 0 \quad \text{dynes}$$

or

$$-mar\epsilon^{j\omega t} + m\chi(r)\ \cos\theta\ \epsilon^{j\omega t} + m\omega^2 r\epsilon^{j\omega t} = 0 \qquad \text{dynes } \angle$$

whence

$$\omega = \sqrt{a - \cos\theta \cdot \chi(r)/r} \qquad \text{radians/sec.} \quad (17)$$

Considering the velocity pair, the first member is

$$Of' = -\Gamma v = -2m\gamma v = -j2m\gamma\omega r\epsilon^{j\omega t} \qquad \text{dynes } \angle$$

The second member is the T component:

$$OT = -jm\chi(r)\ \sin\theta \cdot \epsilon^{j\omega t} \qquad \text{dynes } \angle$$

For equilibrium

$$Of' + OT = 0$$

or

$$-j2m\gamma\omega r\epsilon^{j\omega t} - jm\chi(r)\ \sin\theta \cdot \epsilon^{j\omega t} = 0$$

From which

$$\chi(r)\ \sin\theta = -2r\gamma\omega \qquad \text{dynes} \qquad (18)$$

and

$$\omega = \sqrt{\omega_0^2 + \gamma^2 \cot^2\theta} + \gamma \cot\theta \qquad \text{radians/sec.} \quad (19)$$

It follows that ω, the new angular velocity under reinforcement, is independent of the force function $R = m\chi(r)$, and depends only on the natural angular velocity ω_0, the phase retardation θ of the restoring force and the magnitude of the damping coefficient γ. Some curves of ω as a function of θ for four particular values of Γ between 50 and 500 dynes per cm. per sec.; *i. e.*, of γ between 500 and 5,000 dynes per cm. per sec. and per gm., are given in Fig. 17. It may be seen that for all values of the damping, $\omega = \omega_0$ for $\theta = 270°$. That is, the angular velocity of reinforced motion is the same as that of unretarded motion when the restoring force is applied at 270° of phase lag, or exactly in phase with the velocity, as seen in Fig. 16. If the phase retardation θ is between 180° and 270°, the new angular velocity will be greater than the natural angular velocity ω_0; but if θ is between 270° and 360°, must be less than ω_0.

Applying the above principle to the corresponding case of reinforced vibration, by taking the projections or real parts of the rotat-

ing vectors, it follows that if any automatically reinforced vibrating system, such as an electromagnetic bell, electromagnetic tuning fork, or humming telephone, is propelled by an elastic force proportional to the displacement, reinforced by a cyclic force some function of the displacement, and damped by a force proportional to the velocity, it is subject to equations (17), (18) and (19) which appear to be new.

In the series of measurements on the humming telephone above

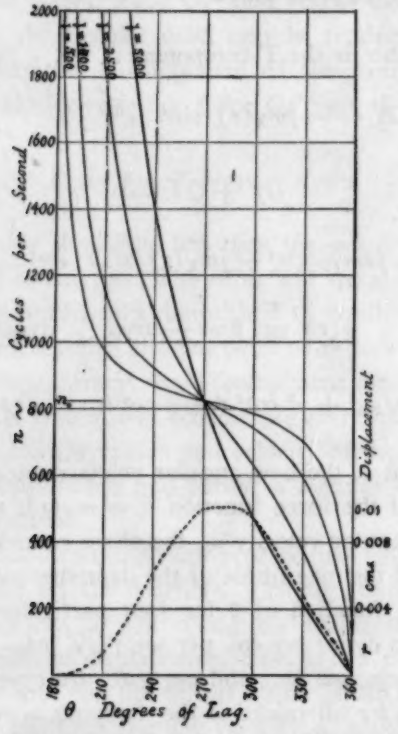

FIG. 17. Reinforced Frequency in Relation to the Phase of the Reinforcement.

outlined, the force function $R = m\chi(r)$ was not measured. The restoring electromagnetic force on the receiver diaphragm, due to the action of the transmitter, will manifestly diminish when the tube-length is increased. For a fixed tube-length, moreover, it cannot increase indefinitely in simple proportion to the displacement of the diaphragm, or to its amplitude of vibration. If we assume provisionally that R increases as the square root of the amplitude of

receiver-diaphragm vibration; so that for a fixed tube-length, $R = bm\sqrt{r}$; *or* $\chi(r) = b\sqrt{r}$; where b is the numerical constant 1.036×10^6 dynes per gm. and per $\sqrt{\text{cm.}}$; then for the example already considered, if $\Gamma = 100$; or $\gamma = 1{,}000$, we find:

The displacement $r = 0.01$ cm. when $\theta = 270°$.

The reinforced angular velocity is $\omega = 5{,}184$ radians per second.; $n = 825 \sim$.

The maximum cyclic values of the vibratory—

velocity	$v =$	51.84 cm. per sec.
acceleration	$c =$	268,740 cm. per sec.^{2}
damping force	$Of' =$	5,184 dynes.
restoring force	$OT =$	5,184 dynes.
elastic force	$OF' =$	13,435 dynes.
inertia force	$Of =$	13,435 dynes.

As the phase θ of reinforcement changes from 180° to 360°, the line of $\gamma = 1{,}000$ in Fig. 17 shows the change in frequency; while the dotted line indicates the computed amplitude of vibration, which reaches a maximum near 280°.

According to the theory, therefore, if the phase of the displacement is 270° behind the displacement of the receiver diaphragm, the reinforced frequency coincides with the natural frequency. This condition is substantially borne out in all of the observations. For example, in Fig. 2, taking the pitch line No. 1, with a natural frequency of $n_0 = 825 \sim$ and a sound-velocity in air of 33,000 cm. per sec., the wave-length $\lambda = 33{,}000/825 = 40$ cm. corresponding to 360° of phase. A lag of 270° would be represented by 30 cm.; so that we should expect the reinforced frequency to be $825 \sim$ at 30 cm. of tube-length, and at every 40 cm. beyond; *i. e.*, in accordance with the series $30 + 40m$, as was substantially observed. Moreover, by reversing the receiver terminals, the phase of the reinforcement is necessarily changed 180°; so that with this change of connection, 270° of phase lag would be altered to 90° of phase lag, or 10 cm. of tube-length. The natural frequency of $825 \sim$ should then occur in conformity with the series $10 + 40m$ cm., as was substantially observed.

When the phase retardation θ of the restoring force (Fig. 16) is less than 270°, we should expect, according to the theory, that the pitch should rise; because the elastic resilience of the diaphragm is virtually increased by the OS component of the new force, and when $\theta > 270°$, on the contrary, the pitch should fall. This was always the case in the observations. We have to bear in mind, however, that with any given tube-length, an alteration of pitch involves a change of wave-length, and therefore a change of phase in transmission through the air-column, besides any electrical change in phase due to change in current frequency. In Figs. 3, 4 and 6, the sloping dotted lines are drawn to indicate constant acoustic phase retardation of 270° for all of the frequencies within the range considered. Taking, for instance, Fig. 6, the break at P occurred 103° in phase from the dotted line of 270°, and the return at S occurred 77° in phase from the dotted line. According to the theory, assuming no electric change of phase, each of these angles should be something less than 90°, since the phase retardation must be something more than 180° on the side of increasing pitch. The discrepancy here is not serious; for the mean of the two angles is 90°. At T and W, however, the corresponding angles are 153° and 119°, with a mean of 136°, which should be something less than 90°, a greater divergence from the theory than observation errors can explain. While, therefore, the theory accounts for all of the experimental results in a general way, it can only be regarded as a first approximation. For example, it is possible that superposed harmonic currents might have to be considered; or that in estimating the damping forces, the inclusion of higher powers of the velocity than the first might be necessary.

Setting aside unexplained deviations, as the tube-length is shortened from a point of $\theta = 270°$, the phase retardation of the electromagnetic reinforcement on the receiver diaphragm is diminished. This causes the pitch to rise, and incidentally readjusts the phase change to a lower value than if the pitch were kept steady. The amplitude of vibration diminishes until the diaphragm suddenly selects a lower pitch for the same tube-length, to which the amplitude will be greater. In other words, the receiver diaphragm automatically seeks to maintain the greatest amplitude that the condi-

tions of reinforcement will permit. If a lower tone, with a phase lag θ more than 270°, will give more amplitude than the higher note to which it has been driven, with θ less than 270°, it will break pitch downwards. This process will continue down to the first wave-length of tube, or 40 cm. in the case examined. For connection *I* of the receiver, it can break to no lower note after passing 270°, and the tone will rise to such a pitch that the amplitude becomes insufficient to excite the transmitter, so that silence should ensue at or near the length 12 cm., as actually observed in Fig. 3. The curve *I* of frequency between 12 and 50 cm. accords fairly well with the curve $\gamma = 1{,}000$ in Fig. 17.

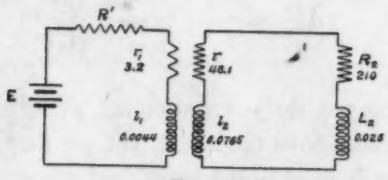

FIG. 18.

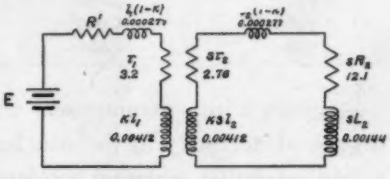

FIG. 19.

FIG. 18. Diagram of Electrical Connections with Step-up Induction Coil.
FIG. 19. Equivalent Diagram of Connections, with Level Induction Coil.

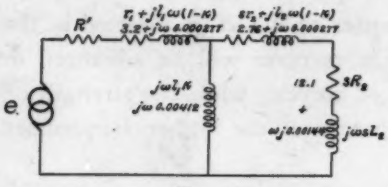

FIG. 20.

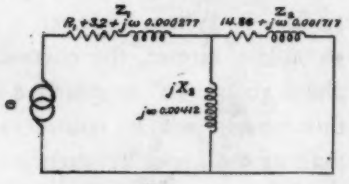

FIG. 21.

FIGS. 20 AND 21. Equivalent Conductive Connections with Level Induction Coil and Alternating E.M.F.

With reference to the influence of capacity in the secondary circuit, Figs. 18 to 21 show the successive steps by which the secondary circuit may be treated as a conductive branch of the primary circuit. Using the constants given in Table I., ignoring any capacity existing between the windings of the coils, and assuming that the effect of the transmitter in the primary circuit is equivalent to an alternating e.m.f. *e*, working through a transmitter resistance *R*, of 50 ohms, we find a coupling coefficient for the coil of $K = 0.937$, and

an inductance ratio $S = 0.0575$ between primary and secondary windings. Proceeding in this way, the following table has been arrived at, giving the conductances which when multiplied by the equivalent transmitter e.m.f. yield the current strength in the secondary circuit of Fig. 21.

TABLE III.

Frequency Cycles per Second. n.	Angular Velocity Radians per Second. ω.	Capacity in Secondary Circuit. μf.	Conductance of Secondary. Mhos.
637	4,000	∞	0.009 $\angle 23°$
796	5,000	∞	0.0087 $\angle 17°$
956	6,000	∞	0.0093 $\angle 12°$
637	4,000	0.2	0.0054 $\angle 140°$
796	5,000	0.2	0.0095 $\angle 123°$
956	6,000	0.2	0.11 $\angle 115°$

Although the assumptions employed do not anticipate a high degree of accuracy in the conclusions above tabulated, yet we may safely infer that when no condenser is used in the secondary circuit (μf. $= \infty$), the secondary current will lead the impressed primary e.m.f. by a small angle, and this current will have substantially the same strength and phase for all frequencies between 600 $\sim$ and 1,000 $\sim$. When, however, a condenser of 0.2 μf. is inserted in the secondary circuit, the current in the receiver will be advanced in phase about 110° or nearly a third of a cycle; while the strength of this current will be considerably greater at the higher frequencies than at the lower frequencies.

Since the total lag in phase of the restoring electromagnetic force behind the displacement of the receiver diaphragm includes (1) the electric current lag; (2) any hysteretic electromagnetic lag in the receiver cores; (3) any mechanical inertia lag of the transmitter diaphragm; (4) the acoustic lag in the air column of the tube; it follows that the total lag with a condenser of 0.2 μf. should be about 110° less than with short-circuited condenser; while the higher frequency notes should be favored, and the lower frequency notes disfavored. Fig. 9 shows that both these effects took place, the acoustic lag had to be increased by about 12 cm., or about 110°, in order to produce mean frequency, and compensate for the current lead.

Also the range of frequency is moved bodily towards higher notes.

All of the experimental series of observations appear to be accounted for and explained by the above theory to a first approximation; although in matters of quantitative detail there remains much room for further development.

In conclusion, the authors desire to express their indebtedness to the Western Electric Co. for the loan of apparatus used in the tests.

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ON THE AFTER-IMAGES OF SUBLIMINALLY COLORED STIMULI.

By EDWARD BRADFORD TITCHENER and WILLIAM HENRY PYLE.

(*Received July 23, 1908.*)

We attempt, in the present paper, to answer the question whether a subliminally colored stimulus may arouse a colored, negative or complementary after-image. This question has been answered in the affirmative both for direct and for indirect vision, and in indirect vision for all three of the retinal zones. Our own experiments, on the other hand, have led us to answer it in the negative. Provided that the subliminally colored stimulus appears on a neutral (black, gray or white) background, and provided that the retina is achromatically adapted, we find no trace of the colored after-image in either direct or indirect vision, with either light or dark adaptation.

Previous Experiments.

1. *Direct Vision.*—In a paper entitled *Das Anpassungsproblem in der Physiologie der Gegenwart* (1904), A. Tschermak compares the course of excitation in the retina with the effects produced by the constant current in a nerve-muscle preparation. The passage is as follows:

"Haben wir doch gerade in der Anwendung des constanten Stromes auf Nerv und Muskel ein vorzügliches didaktisches Mittel, um die Grundbegriffe der allgemeinen Reiz- und Adaptationslehre zu veranschaulichen und einzuprägen. Am besten demonstrieren wir als Gegenstück zugleich die Wirkung eines mässig satten Farbglases auf das Auge: die Phase der Reizwirkung, individuell verschieden lang, und dadurch erinnernd an die verschiedenrasche Adaptation des Praeparates vom Warmfrosch und Kaltfrosch an den constanten Strom — weiterhin das Stadium der vollendeten Adaptation, endlich den gegensinnigen Oeffnungseffect. Nicht minder lehrreich ist die Parallele des subjectiven und des objectiven Erscheinungsgebietes für das Phaenomen des Einschleichens d. h. des Ausbleibens einer sinnfälligen Reizwirkung, wenn der Reiz so langsam anwächst, dass das Adaptationsvermögen folgen kann — gleichwohl hat auch nunmehr Wegfall

des 'Reizes' eine gegensinnige Oeffnungswirkung. Analoges gilt vom Ausschleichen, also vom Ausbleiben eines sinnfälligen Oeffnungseffectes. Zum optischen Versuche schiebt man zweckmässig eine schwach tingierte Glasplatte vor die andere oder benützt einen Keil farbigen Glases."[1]

The observation here briefly mentioned was apparently made in light-adaptation. The observer, we may suppose, looked through a vertical slit in a cardboard screen towards a window. The thin end of the colored glass wedge, viewed through the slit, appeared colorless. The wedge itself was slowly pushed forward — so slowly that progressive adaptation prevented its color from being perceived. Presently the observer turned his eye to the cardboard screen, and there saw the negative colored after-image, the "gegensinnige Oeffnungswirkung" that followed the "Einschleichen des Reizes."[2]

2. *Indirect Vision.*—In the Studies from the Psychological Laboratory of Mount Holyoke College for 1905, Miss G. M. Fernald reports the arousal of colored after-images in the peripheral or black-white zone of the retina. "A further point worth mentioning"—so the passage runs—"is the fact that, in the case of several colors, exposure, beyond the limits where any color is seen, is followed by a very clear [colored] after-image. This was repeatedly found to be true with red, orange, green and blue and often with yellow [stimuli]. This after-image for the first three and for yellow was blue, and for blue a very clear yellow. This may explain the 'gegenfarbige' zone found by Hellpach in his dark-room work, as under those conditions there would have been no way of telling whether the color came exactly at the time of exposure or immediately afterwards."[3] No further details are given.

[1] *Archives des sciences biologiques,* XI., Supplément (*Festschrift* for Professor J. P. Pavloff), 82 f.

[2] The procedure is sketched by H. Abels, *Zeits. f. Psychol.,* XLV., 1907, 86. "Man kann . . . einen schwach gefärbten Glaskeil so langsam vor das Auge schieben, von der Kante gegen den Rücken fortschreitend, dass überhaupt keine Farbenempfindung zustande kommt; und dennoch haben wir bei plötzlichem Entfernen desselben und Betrachten einer indifferent gefärbten Fläche die deutliche Empfindung der komplementären Farbe." Abels is here quoting a conversation with Tschermak; there is no evidence that he himself performed the experiment.

[3] "The Effect of the Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision," *Psychol. Review,* XII., November, 1905, 405.

These observations would, no doubt, have been repeated, and their interpretation discussed by other experimenters, had not Baird published, earlier in the same year, his study of the color sensitivity of the peripheral retina. "There seems to be no doubt," Baird had written, "that Hellpach's zone of complementariness is an artifact, and that its discovery is wholly due to the experimenter's failure to avoid retinal fatigue [chromatic adaptation] in his explorations." [4] Nevertheless, one of the present writers (*T*) made in 1906 a fairly long series of campimetrical observations (some 200 in all) with the view of testing Miss Fernald's conclusion. The colored stimuli were Hering papers, R, Y, G and B; the backgrounds were white, neutral gray and black. In no case was "exposure, beyond the limits where any color is seen," followed by a colored after-image, clear or obscure. All four colors, if they gave an after-image at all, gave a colorless image, indistinguishable from the after-images of gray stimuli—as these gray stimuli themselves were indistinguishable from the colored papers. It therefore seemed probable—indeed, it seemed practically certain—that the Mount Holyoke results were due to a defect of method. Since Baird's disproof of the "gegenfarbige Zone" was deemed complete and final, the Cornell observations were not published.

However, in the following year, 1907, a second paper from the Mount Holyoke laboratory reported the same phenomenon. "At the extreme periphery it sometimes happened: (*a*) that a stimulus which was clearly seen produced no after-image. . . . (*b*) On the other hand there were 118 cases in which a subliminal stimulus produced an after-image which was perfectly distinct in color. . . . That this somewhat unusual result was not the outcome of imagination or suggestion seems proved by the fact that these invisible colors gave rise to their appropriate after-images." [5] The authors, the Misses H. B. Thompson and K. Gordon, found no indication of Hellpach's zone of complementarism. They refer the images to the enhancing influence of a light background.

[4] J. W. Baird, "The Color Sensitivity of the Peripheral Retina," Carnegie Institution of Washington, Publication No. 29, May, 1905, 73.

[5] "A Study of After-Images on the Peripheral Retina," *Psychol. Review*, XIV., March, 1907, 126 f., 129 f.

Again, in 1908, in a continuation of her former study, Miss Fernald writes: "In agreement with the observations already made in our first paper, and later in the work of Miss Thompson and Miss Gordon, our results show that in many cases a characteristic colored after-image follows an unperceived color stimulus. In general this after-image is perfectly clear and distinct. . . . That the phenomena here described are genuine after-images is shown by the fact that the color is in every case the color complementary to the stimulus as [it would be] perceived either in central or in peripheral vision, although the observer was kept in complete ignorance concerning the nature of the stimuli employed, and so had no clew as to what after-image was to be expected in cases in which the [color of the] stimulus was not seen. Moreover, gray and white, though frequently used as stimuli, were never followed by colored after-images." [6] Hellpach here drops out of sight altogether, while the range of the subliminally aroused after-image is extended, from "the extreme periphery," to include both the B–Y and the R–G zones.

New Experiments.

I. *Direct Vision: (a) Light-Adaptation.*

Experiment I.: The Glass Wedge.—We wished to begin our own experiments by repeating Tschermak's observation with the faintly colored glass wedge. However, the difficulty of finding a suitable glass proved to be so great that this Exp. I. was, as a matter of fact, performed last of all. After many delays we were able, through the kind assistance of Professor J. A. Brashear, to secure a wedge of light blue glass, 5 by 20.5 cm., the thin end of which was almost colorless in clear daylight. Although the color might well have been still fainter, we found it possible, with an observation-slit of 22 by 5 mm., and with a white muslin screen stretched between the glass wedge and the white-screened windows from which our illumination was derived, to take observations of 2 to 5 min. duration, in which the wedge was moved, for the practised observers, from 1.5 to 4 cm., and for the unpractised from 5 to 10 cm.

[6] "Studies from the Bryn Mawr College Laboratory: The Effect of the Brightness of Background on the Appearance of Color Stimuli in Peripheral Vision," *Psychol. Review*, XV., January, 1908, 33 ff.

We made no long series of tests, since the question at issue had already been answered, so far as we could answer it, by the following Exps. II.–V. The experiments were, however, carefully conducted. The work was done in a long gray-tinted light-optics room, with achromatic adaptation; the observers were the writers (*T*, *P*), Mr. L. R. Geissler (*G*), assistant in psychology, and two unpractised students, Mrs. G. L. de Ollogni and Mr. E. M. Stevens; and the experimenter had acquired great skill, from Exps. II. and IV., in moving the wedge slowly and steadily forward. In general, the stimulus-background was black, and the field for the projection of the after-image was white, though these relations were occasionally changed.

As we had expected, there was no trace of color in the after-image; this result was uniform. In control experiments, in which (after a period for the recovery of the eye) the glass was exposed for 30 sec. at the point finally reached in the adaptation experiments, the after-image showed a brief period of dirty orange or brownish yellow, followed by gray.

Experiment II.: The Marbe Color Mixer.—The observations with Tschermak's wedge could not, in any case, be regarded as more than preliminary. For systematic work we employed, first, the Marbe color mixer, which permits the change of a colored sector during rotation of its discs, and thus gives scope for progressive adaptation.

The observer, head in rest, was seated at a distance of 1 m. from a black cardboard screen. The rotating discs were observed through a circular opening, 2 cm. in diameter, cut in the screen at the level of the eyes. The observation was monocular, and was continued for 5 to 7 min. The discs were made up of white, with a sector of colored paper (Zimmermann R, Y, G, B, V); the color at the outset was subliminal for the achromatically light-adapted eye, and was gradually increased in amount as the observation proceeded. The after-image was projected upon a fixation-point marked on a white cardboard dropped in front of the black screen.[1]

[1] For comparative purposes, a few observations were taken with a gray screen, and with projection upon a black or gray background. Nothing new resulted.

The regular observers were *T, P, G,* and Mr. T. Nakashima, graduate scholar in psychology (*N*). A few observations were secured from Professor I. M. Bentley (*B*), and from an unpractised observer, Mr. H. J. Bool; single observations were made by several visitors to the laboratory.

In intention, the procedure was without knowledge. In practice, the experimenter found it impossible, in the early stages of the work, to regulate the size of the colored sector in precise accordance with the course of adaptation. The observer was therefore instructed to tap on the table with a pencil whenever he perceived a color in the stimulus. If a tap was given, the experimenter ran the colored sector back through five or ten degrees, and continued the experiment from that point. The results of these interrupted observations varied, according to the frequency of the taps and the insistence of the color in the stimulus. The following are typical records.

A. No Color Seen in Stimulus.

Observer.	Color in Disc.	After-image.	Duration of Obs.
T	205° B	Gray	7 min.
	180° V	Gray	6 min.
P	120° G	Gray	5 min.
	140° V	Gray	5 min. 15 sec.
G	135° R	Gray	6 min.
B	190° B	Gray	6 min.

After a period for recovery, the stimuli were exposed at their final color-strength for 30 sec., and the after-image was projected as before. The results, in the above instances, were as follows:

Observer.	Color Seen.	After-image.
T	Blue	Brownish yellow
	Bluish violet	Dirty olive yellow
P	Green	Pink
	Bluish violet	Dingy yellow
G	Red	Gray
B	Blue	Clear yellow

B. Color Seen in Stimulus.

Observer.	Color in Disc.	After-image.	Duration of Obs.
T	140° G	Gray	5 min.
G	120° Y	Dark blue	5 min.
	220° B	? Orangish	7 min.
N	155° R	Gray	7 min.
	130° Y	Blue	6 min.

The control experiments, with 30 sec. exposure, gave the results:

Observer.	Color Seen.	After image.
T	Green	Purple
G	Yellow	Dark blue
	Blue	Yellow
N	Pink	? Violet
	Yellow	Blue

The general results of these experiments may be summed up in the following propositions.

1. With every one of our observers, regular and casual, we have been able to raise a color-component in the stimulus from a subliminal to a normally supraliminal value, while the stimulus appeared throughout as gray. In no instance of this kind has the observer found the complementary color in the after-image. Our results thus stand in direct opposition to the observation of Tschermak.

2. There are, however, marked individual differences among the observers. In the 7 min. which represented the limit of our observations, it was difficult, with *G* and *N*, to increase the color-component, without detection, to a normally supraliminal amount: with *T*, *P* and *B* there was no such difficulty. The control images, on the other hand, were obtained most readily from *T* and *P*.

The observer *N* is of the subjective type, and is often misled by an "expected" or "imagined" color. Thus a disc containing 175° G was seen as B with a rim of Y; the after-image, after 6 min., was a Y of irregular form, larger than the stimulus. We recur to these "imagined" colors later. The remaining observers were of a distinctly objective type.

3. There were also, as might be expected, marked differences in the "coloring power" of the Zimmermann papers. Experiments of the form *A* were easiest with B, less easy with V; then follow in order R, G, Y. The last-mentioned color, indeed, gave results only with entirely naive and unpractised observers. The R and G, when seen as color, usually appeared first as Y.

4. As a rule, the after-images, whether colored or gray, developed very slowly. The gray images, in particular, might appear only after a blank interval of 15 to 30 sec. They usually showed

two stages, dark and light. The colored images, both of the regular and of the control experiments, passed off as gray.

Experiment III.: The Color Mixer with Unchanged Discs.—So far we have followed and systematised Tschermak's method; the amount of color in the stimulus has increased, during the single observation, and has been compensated by a progressive adaptation. In the present experiments the amount of color in the discs is increased from subliminal to normally supraliminal, step by step, in successive observations.

The rotating discs were observed, as before, through a circular opening in a black or neutral gray screen. The discs themselves were made up of neutral gray (identical with that of the screen), with a colored sector (Hering R, Y, G, B). The stimulus was fixated for 1 min., and the after-image was projected upon a neutral gray or black background. *P, G* and *N* served as regular observers: a few observations were also taken from *B* and *T*. The following are typical results.

Observer.	Color in Disc.	Color Seen.	Color in After-image.
G	4° G	None	None
	10° G	Green	None
	14° G	Green	Pinkish
N	9° B	None	None
	12° B	None	None
	20° B	? Pinkish	None
	50° B	Blue	Yellow
P	6° R	? Ruddy	None
	10° R	Red	None
	30° R	Red	Green
T	6° Y	None	None
	12° Y	? Yellowish	None
	20° Y	Yellow	Dark blue

In the above observations, the black screen and the neutral gray background were employed. Other arrangements of screen and background gave similar results.

In no case was a colored after-image obtained from a subliminally colored stimulus. On the contrary, the image appeared only when the stimulus-color was distinctly supraliminal.

(*b*) *Dark-Adaptation.*

Experiment IV.: The Glass Wedge.—Besides furnishing the light blue wedge of Exp. I., Professor Brashear supplied us with smaller and more highly colored wedges of claret, red, orange, green and blue glass. With these, or with combinations of them, we proceeded as follows.

A sheet of ground glass was inserted in the Hering window of a large dark-room: the width of the strip could be regulated at will. Some 2.50 m. before the window was a table, on which stood a large screen of white cardboard. Immediately behind a vertical slit in this screen (3 by 25 mm.) lay a grooved strip of wood, in which the wedge or wedges could be moved. Observations were made in dark-adaptation. The thick end of the wedge was first shown; it appeared as black or as dark gray. The wedge was then moved along, very slowly: if the observer saw its color, he tapped with a pencil, and the experimenter withdrew it a trifle, to start again after a few seconds. At a given signal, the observer looked away from the slit to the cardboard screen, or to a black surface directly below the screen, and watched the development of the after-image. The regular observers were *T, P, G* and *N;* a few observations were also made by *B.*

Owing to the difficulty of procuring the large glass wedge of Exp. I., these dark-room observations were the first taken. And, in our desire to do justice to Tschermak's method, we spent more time and trouble upon them than we like to recall. The observer's head was fixed securely in a head-rest; the height of the screen was carefully adjusted; generous time was allowed for adaptation; the admission of light was rigorously controlled, beforehand, by the experimenter; the uniform movement of the wedge was assiduously practised. We were rewarded, however, by the unequivocal character of the results. Though observation might be continued for 5 min.; though during this period the observer might tap his glimpse of color no less than seven times; and though in the control experiments, with immediate observation of the part of the wedge finally exposed, a good complementary after-image might be obtained in

20 sec.: we did not once, in the course of the principal experiments, obtain a record of color in the after-image. Sometimes the after-image failed to appear at all; more often it appeared, and obstinately remained, as gray.

The duration of a single observation varied between the limits of 2 min. 30 sec. and 5 min.; most of the exposures were about 3 min. The number of taps varied from 0 to 7; the average for all observers was 4. The color was thus much more insistent than in Exp. II. — partly, no doubt, because the range of possible movement was only about one-third of that allowed by the Marbe mixer. In the control experiments, *T* and *P* obtained the colored after-image fairly easily; *G*, *N* and *B* often failed to secure it.

Experiment V.: Colored Papers.—These observations were also made in the dark-room and with dark-adaptation. A number of Milton-Bradley colored papers, 4 by 8 cm., were pasted upon white, neutral gray and black grounds. The Hering window was so adjusted that, for the experimenter, the color of the particular paper exposed was just subliminal. The observers (*T*, *P*, *G*, *N* and occasionally *B*) fixated the colored strip at a distance of 1 m. for 40 sec., and projected the after-image upon a white, neutral gray or black surface. All possible combinations of stimulus-ground and projection-ground were employed.

The observer was instructed to report the quality of the stimulus as it appeared at first fixation, and to mention any qualitative change that it might undergo in the course of an observation. In most cases the color was subliminal; and the subliminally colored stimulus never gave a colored after-image. In the cases in which the color of the strip was seen, the after-image was sometimes colored, sometimes gray.

The direct judgment of color under these conditions is extremely difficult, and the observer is sorely tempted to avail himself of secondary criteria—brightness, velvetiness, depth, shimmer, etc. An observer of the objective type soon learns, however, to distinguish between vision and imagination: "I can see nothing," he will say, "but I should guess that it is red" or what not. The guesses were confined—probably from the analogy of the immediately preceding Exp. III.—to the four colors R, Y, G, B; and, as we had the full set of Milton-Bradley papers at our disposal, they were more

often wrong than right.[8] Their influence upon the after-image appeared only in the case of the subjective observer *N*. Thus, R seen on W was judged by *N* to be "red or blue"; and the after-image, also on W, was a large irregular disc of yellow. R seen on Bk was judged to be "bluish"; and the after-image, on gray, was green-blue with a vague yellow rim. B seen on W was judged "blue or red"; and the after-image, on gray, was red above and blue below, with a yellow patch between. It is noteworthy that here, as in Exp. II., after-images of the "supposed" or "imagined" color invariably differed in form and size from those of the true color. The observer did not realise the significance of this difference, though in time he would doubtless have learned to use it as a secondary criterion.

II. *Indirect Vision.*

We have already mentioned the experiments made by *T* in 1906 with the view of testing the conclusions of Miss Fernald's first paper. The observations were rigorously confined to the Bk–W zone, and their outcome was definitely negative. In the meantime, however, the arousal of a colored after-image by a subliminally colored stimulus had been maintained for both the B–Y and the R–G zones. Unsystematic observations made in the Cornell Laboratory failed to confirm this result. It seemed worth while, however, to obtain further testimony; and Professor J. W. Baird, of the University of Illinois, very kindly consented to investigate the subject.[9]

[8] One of the observers remarked that the experiments showed—what he had never fully understood before—how it is that a case of partial color-blindness may remain undetected both by the color-blind person himself and by the normal persons in his surroundings. In principle, the remark was correct enough; but in practice the observer would have had to revise and extend his criteria very considerably.

[9] All the observations in indirect vision mentioned in this paper were carried out with light-adaptation. Peripheral after-images in dark-adaptation are practically non-existent. In *op. cit.*, 56 f., Baird writes: "After-images—in the ordinary sense of the term—were almost invariably absent from our experiments. They were reported in less than one per cent. of our exposures; and when they did occur, they were aroused by the stimulation of paracentral, never of peripheral, regions of the retina." And in a personal letter he adds: "There is an interesting difference of function in

The experiments were carried out by means of a simplified form of the Zimmermann perimeter, which permitted an accurate record of the degree of eccentricity at which the stimulus was exposed. Exploration was confined to the horizontal nasal meridian of each eye. The stimulus was a beam of light from an electric (16 c. p.) lamp, transmitted through appropriate combinations of gelatines and colored glasses; the colors employed were (non-equated) B and Y, R and G. Six of the most reliable laboratory students[10] acted as observers, and Professor Baird had personal charge of the entire work. The after-images were projected upon white, gray and black grounds. The experiments proper were preceded by a careful determination of the outermost limits of color vision for the stimuli used, and all pains were taken to avoid chromatic adaptation.

The following may serve as a sample of method and results.

Determination of Outermost Limits of Blue Vision: Observer Bu.
Right Eye.

Preliminary.		Series 1.	
90°–75°	Nothing	72°–58°	Black
70°–55°	Dark gray	56°–50°	Bluish
50°–45°	Bluish	48°–	Blue
40°–	Blue		

Series 2.		Series 3.	
75°–63°	Black	73°–61°	Very dark gray
61°–53°	Bluish	59°–	Bluish
51°–	Blue		

Outermost limit (bluish or blue): 61°.

Left Eye.[11]

90°–65°	Nothing	62°–50°	Black
60°–40°	Dark gray	48°–44°	Bluish
35°–	Bluish to blue	42°–	Blue
60°–48°	Black	58°–44°	Black
46°–	Bluish	42°–	Bluish

Outermost limit (bluish or blue): 48°.

the peripheral retina in light-adaptation and in dark-adaptation. In the latter case, after-images—both uncolored and colored—are faint or wholly lacking. In the former case they are readily perceptible. Yet even in light-adaptation they are less perceptible than are the primary images aroused by the given stimuli."

[10] The Misses M. Miller, A.B., and B. Scoggin: and Messrs. C. B. Busey, A.B., R. Garrett, O. L. Herndon and A. C. Schertz, A.B.

[11] The visual acuity of the left eye was less than that of the right.

Perimetrical Experiments.

Stimulus.	Duration.	Perception.	After-image.
90° Right	30 sec.	Nothing	None
90° Left	30 sec.	Nothing	None
80° Right	30 sec.	Nothing	None
80° Left	30 sec.	Nothing	None
70° Right	40 sec.	Dark gray	None
70° Left	40 sec.	Nothing	None
60° Right	40 sec.	Bluish, then black	None
60° Left	40 sec.	Gray	None
50° Right	40 sec.	Dark bluish, then gray	Yellowish, then gray
50° Left	40 sec.	Trace of bluish, then gray	Gray

It does not seem necessary to print the full set of results, though the data are at the disposal of anyone who may wish to consult them. The net outcome of the enquiry, in Professor Baird's words, is as follows: "In not a single instance did any stimulus give a colored after-image at a retinal region where it gave an uncolored image," *i. e.,* where it was seen as black or gray. He proceeds: "I have tried every variation of the conditions (with exclusion of chromatic adaptation) which my ingenuity could devise; and the result is in every instance negative, so far as the contention of the Misses Fernald, Thompson and Gordon is concerned."

Criticism and Interpretation.

1. The positive outcome of Tschermak's observations with the glass wedge must, in our opinion, be explained by the prepossession of the observer and the roughness of the method employed. Had Tschermak been in doubt as regards the after-image, he would have had recourse to a more refined instrument, as the Marbe color-mixer. And had he adopted a better method, we cannot doubt, on our side, that the outcome of his observations would have been negative. We may, perhaps, venture to express the hope that he will now submit his hypothesis to a stricter test.

2. It is less easy to account for the peripheral results. The *experimentum crucis,* in positive regard, would seem to be the production of a colored after-image, in the achromatically adapted eye, at a point lying well beyond the limits of B–Y vision. It must be remembered that in all liminal determinations an unnoticed variation in physical or physiological conditions, or in the conditions of

attention, may lead to a serious variation of numerical result. It is, for instance, exceedingly doubtful if any but the most careful and most highly practised observers can maintain their fixation so accurately as to ensure a precise localisation of the retinal area affected by a given stimulus. Moreover, we are here dealing with a retinal function which tails off gradually from center to periphery: so that a very slight shift of regard, or a momentary lapse of attention, or a minimal change in adaptation or in illumination may be enough to vitiate an observation. An illustration may be taken from the records of the observer *Bu.*, quoted above. The outermost limit of B-vision, in the left eye, was determined as 48°. Nevertheless, the observer reported, in the experiments proper, a "trace of bluish, then gray" with the stimulus at 50°. There was no colored after-image. But suppose a tinge of blue-adaptation: then we might have had a perception of gray, and a yellow after-image; and we should still have been, apparently, beyond the limit of B–Y vision. It was only the care taken to avoid chromatic adaptation that prevented the positive result.

It is, of course, precisely this crucial experiment which is described affirmatively by Miss Fernald in 1905,[12] and which came out negatively in *T*'s experiments of 1906. The question then arises as to the accuracy of determination of the zonal limits. And on this point we may quote specimen results from Miss Fernald's tables.

1. R stimulus on light gray background.[13]

10°–73°	Stimulus uniformly seen as red.
74.5°	*No color seen.*
76°	Red seen in two observations.
80°	Red seen in four, no color seen in two observations.
82.5°	No color seen.
84°	No color seen.

[12] We follow the phrasing of the *Psychol. Review* of 1905: "Exposure, beyond the limits where any color is seen, is followed by a very clear after-image." In the *Journ. Philos., Psychol. & Sci. Meth.*, iii., 1906, 352 (Report of Sec. of N. Y. Acad. of Sciences), the report reads: "After-images were perceived, almost without exception, as far out as any color could be distinguished, and in many cases were clearly seen though the stimulus color was not recognised."

[13] *Psychol. Review*, XII., 408. Italics ours.

85.5° Red seen once, no color seen once.
87° *Red seen twice,* no color seen twice.

The conditions can hardly have remained constant from 74.5° to 87°. Again, R on Hering gray no. 7 is seen colorless at 37°, while it is seen red at 39°, 41.5° (twice), and even at 47° (twice).[14] And yet again, G on the same gray is seen colorless at 82°, green at 84°, and once colorless and once green at 87°.[15] Instances of this irregularity might easily be multiplied.

2. If we turn to the special table for the limits of B and Y, we find a greater uniformity of result, but a certain arbitrariness in the selection of the limiting values. Thus, on various backgrounds and for different observers, the limits for Y are taken as

(*a*) 97°, although at 98.5° the color is seen 3 times out of 14,
(*b*) 88.5°, although at 92.5° the color is seen once in 3 times,
(*c*) 95.5°, although at 98.5° the color is seen once in 3 times,
(*d*) 92.5°, although at 95.5° the color is seen 3 times out of 10,

and so on. Similarly, the limits for B are taken as

(*a*) 88.5°, although at 91.5° the color is seen once in 4 times,
(*b*) 97°, although at 99.5° the color is seen once, and one observation is doubtful,
(*c*) 97°, although at 99.5° the color is seen 3 times out of 9, with one observation doubtful,

and so on.[16]

Now in her second paper, of 1908, Miss Fernald states that the paradoxical after-images "are perceived most frequently either just inside or just beyond the regular limits for the color."[17] If this statement may be applied to the limits of color vision at large, *i. e.*, to the work of 1905, we must conclude that the crucial experiment has not been adequately performed; for the limits given are, as we have seen, irregular and arbitrary.

Each, however, if we maintain that *T*'s results are conclusive for the Bk–W zone, we have still to account for the colored after-images of subliminally colored stimuli in the B–Y and R–G zones.[18] Miss

[14] *Ibid.*, 422.
[15] *Ibid.*, 416.
[16] *Ibid.*, 402.
[17] *Psychol. Review,* XV., 33.
[18] Miss Fernald uses the term "unperceived," not subliminal. The latter word is, however, employed by the Misses Thompson and Gordon, whose results Miss Fernald assimilates to her own. That "unperceived" really means "imperceptible" is shown also by a passage in a letter received from Miss Fernald: "I should be very much afraid of my observer's life, if it depended on his identification of the stimulus color, in all cases in which a clearly colored after-image is seen. In fact, when forced to say what stimulus he thought was used, he guessed at B for O as often as O for O, insisting all the while that he did not see any color."

Fernald has been good enough to send us an account of the conditions under which her observers found the after-image, and to make a special series of observations, with Mr. C. E. Ferree as observer. "The head," she says, "must be held firm (my method is the bit, with the impression of the teeth). The background must be light, and the illumination good. The observer must hold the fixation steadily after the stimulus is removed. The after-image screen must be white to obtain Y or B after-images and black to obtain R after-images. A very slight change in conditions makes a great difference in results, which seem to me to depend wholly on brightness." Professor Baird was acquainted with these conditions before he undertook his perimetrical observations.

The new set of observations is as follows.

Observer: C. E. Ferree. Full illumination on bright day (May 17, 1908). Nasal meridian, right. White ground. Projection field white, except in obs. 14–17, when it was black. Stimulus, 13 sq. mm. Distance from eye to stimulus, 25 cm.

Fixation Point.	Stimulus.	Color Seen.	After-image.
80°	O	Dark gray	Unsaturated light blue
85°	B	Just dark	Wash of unsaturated yellow
85°	Y	Nothing	Nothing
80°	Y	Tinge of dirty yellow	Very pale blue
80°	Medium gray	Dark	White
80°	O	Indefinite gray	Nothing
80°	Light gray	Dark	White
75°	Y	Reddish yellow	Good blue
75°	B	Good blue	Good yellow
75°	B	Good blue	Good yellow
65°	O	Yellowish red	Unsaturated blue
65°	Y	Reddish yellow	Blue
60°	G	Indefinite greenish gray	Uncertain
65°	G	Greenish yellow	Dark red, more saturated than stimulus
80°	Medium gray	Dark	Nothing
80°	Medium gray	Dark	Nothing
65°	G	No color	Flash of red
65°	R	No color	Blue

Positive results occur in the two first and two last observations of the series. The former may be explained in terms of chromatic adaptation. If, as the illumination suggests, the observer began the

work in Y-adaptation,[19] the first, blue after-image would naturally follow. If the second observation was taken at too short an interval of time, the resulting B-adaptation would show itself as a yellow after-image. The two final observations suggest a shift of conditions. G is seen at 65° as greenish yellow, and as colorless; at 60° as indefinite greenish gray. It is possible that, in the case in which "no color" is reported, the G simply escaped notice; peripheral colors at the limit of vision often appear as momentary flashes. Again, R is reported at 65° as "no color," although "reddish yellow" had been seen as far out as 75°. It is possible that the flash of red escaped notice; it is also possible that R-adaptation, from the preceding after-image, brought out the blue.

The puzzling thing is that the positive outcome should be thus definite in the Mount Holyoke and Bryn Mawr laboratories, while neither Professor Baird nor ourselves—though working with full knowledge of conditions, and though trying various possibilities which have not been reported in detail[20]—are able in a single case to obtain the colored after-image. We can only guess at an explanation; and we offer the following guesses in what seems to us to be the order of their likelihood: (1) chromatic adaptation;[21] (2) the momentary and flash-like appearance of colors at the limit of vision; (3) the phenomenon of "fluctuation of attention"; (4) defective method and unsystematic procedure in the determination

[19] These observations were taken "after the limits had been roughly determined in previous experiments." If the determination of limits was made at the same sitting, and if the last test-color employed was O, there would be additional reason for an initial Y-adaptation.

[20] Thus, Mr. Ferree wrote to us: "After-images seem to occur most intensively when the stimulus is removed while adaptation is still going on. If one carries the stimulation to a stationary point in adaptation, the after-image will weaken in proportion to the length of time during which the stimulus is regarded before the after-image is evoked. This is true whether one uses intensive or slightly supraliminal stimuli." We thought that it might possibly be true of subliminal stimuli, and accordingly made brief observations both in light and in dark adaptation. But we never saw the after-image.

[21] On chromatic adaptation, see Baird, *op. cit.*, 57 ff., 64 ff., 73 f.; *Journ. Philos., Psychol. & Sci. Meth.*, II., 1905, 21.

of zonal limits; (5) unnoticed variations, physical, physiological or psychological, in the conditions of observation during a series.[22]

We are well aware that negative experiments are logically inconclusive.[23] The fact that we have failed to find the colored after-image does not prove that this after-image is non-existent. We have, however, attempted a positive explanation: for Tschermak's result, in terms of prepossession and inaccurate method; for Miss Fernald's result, in terms (predominantly) of chromatic adaptation. Further experimentation by other observers must show whether our hypotheses are correct.

We are aware, also, that the charge of prepossession is double-edged, and that we may ourselves be accused of an initial bias. We freely confess that we, as well as Professor Baird, approached the peripheral experiments in a sceptical attitude of mind. On the other side, we may point out that the scepticism was positively based upon the results of Baird's Carnegie Institution research, and that the student-observers at the University of Illinois knew nothing of the question at issue.

In the case of Tschermak's observation, however, our initial bias was positive; we were surprised at the uniformly negative character of our results with the Marbe mixer. Tschermak's position seemed to accord well with current visual theory. Moreover, we knew that a contrast-color may be more saturated, may appear more "real," than the inducing, objective color. We knew that Heymans, in his experiments on "psychische Hemmung," had sometimes seen the contrast-color while the inducing color was still unperceived.[24] We knew of Helmholtz' statement, "dass die gesät-

[22] In a letter to *T* Miss Fernald remarks: "You will see that colored after-images were seen in less than one third of the total number of cases in which the stimulus-color was not seen." In a communication made to Professor Baird, she estimates, roughly, that the phenomenon appeared in about five per cent. of her exposures upon the peripheral retina. This sporadic and fortuitous character of the after-images suggests that they are the product of some variable condition which has not been taken account of in the investigations.

[23] J. S. Mill, "A System of Logic," 1884, 515; W. S. Jevons, "The Principles of Science," 1900, 434.

[24] G. Heymans, Untersuchungen über psychische Hemmung, i. *Zeits. f. Psychol. u. Physiol. d. Sinnesorgane*, XXI., 1899, 328. "Wo mit weissen Sectorenscheiben experimentirt wurde, kam es öfters vor, dass ehe noch der Ring die Farbe des Papierstückes erkennen liess, sich im Hintergrunde schon die Contrastfarbe bemerklich machte."

tigsten objectiven Farben, welche existiren, die reinen Spectralfarben, im unermüdeten Auge noch nicht die gesättigste Farbenempfindung hervorrufen, welche überhaupt möglich ist, sondern dass wir diese erst erreichen, wenn wir das Auge gegen die Complementärfarbe unempfindlich machen." [25] There was, then, no *a priori* reason to doubt Tschermak's result; on the contrary, we thought it probable that under conditions which were unfavorable to the appearance of the stimulus-color, but favorable to the appearance of its complementary, the subliminally colored stimulus would give a perceptibly colored after-image.[26] As a matter of fact, it did not.

[25] "Physiol. Optik," 1867, 370; 1896, 520. *Cf.* W. Wundt, "Physiol. Psychol.," II., 1902, 146.

[26] This possibility was considered, also, in the peripheral work; so that even for that our bias was not wholly negative.

ON THE CLASSIFICATION OF THE CETACEA.

By FREDERICK W. TRUE.

(*Read April 24, 1908.*)

In this communication I wish to call attention to the various changes in the generally-accepted classification of the Cetacea proposed by Professor Dr. O. Abel, of the University of Vienna, in connection with his recent study of the Miocene toothed whales—chiefly those obtained from the vicinity of Antwerp, and now in the museum of Brussels.[1] Professor Abel's classification (1905) is as follows:

Odontocètes
- + Archéocètes
- + Squalodontidæ
- Physeteridæ
- Ziphiidæ
- + Eurinodelphidæ — Argyrocetinæ
- Acrodelphidæ . . . — Acrodelphinæ
- + Saurodelphidæ — Iniinæ
- Platanistidæ — Beluginæ
- Delphinidæ

I would call attention particularly to the following features to which my remarks will mainly relate:

1. The use of the term "Odontocètes" for all toothed whales and zeuglodonts.

2. The subordination of the "Archéocètes" to the "Odontocètes."

3. The new family Eurinodelphidæ.

4. The new family Acrodelphidæ (should be Iniidæ).

5. The inclusion of *Delphinapterus* and *Monodon* in this family instead of in Delphinidæ, and the inclusion of *Stenodelphis* and *Pontistes.*

[1] *Mém. Mus. Roy. Hist. Nat. Belgique,* 1, 1901 and 3, 1905.

6. The new family Saurodelphidæ.

7. The family Platanistidæ, consisting of *Platanista* only.

The zeuglodonts are included in the order Cetacea by the majority of cetologists, though they were rejected from the great "Osteography" of Van Beneden and Gervais, as these authors did not consider them to be cetaceans. Brandt placed them with the squalodonts as families in a tribe subordinate to the Odontoceti,[2] but nearly all other authorities have considered them as a distinct suborder,—Archæoceti, or Zeuglodontes.

It seems to be generally agreed that the zeuglodonts have been proven by the researches of Dawes, Fraas, Stromer, Andrews and others to be derived from the creodonts. I do not know from what particular creodont they are supposed to have sprung, and whether the connection is good in that direction is for those most familiar with the creodonts to decide. The chief argument appears to be that in some zeuglodonts some of the molars are three-rooted.

Whatever may be the truth as regards that connection, various zoölogists have proposed, in more or less definite terms, to unite the zeuglodonts to the ordinary cetaceans through the squalodonts, which are clearly cetaceans, but with two-rooted or three-rooted teeth having serrated crowns. Professor Abel advances the concrete proposition of uniting the zeuglodonts and squalodonts[3] through the small form from the Caucasus, described by Lydekker under the name of *Zeuglodon caucasicus*,[4] and afterward made the basis of a new genus, *Microzeuglodon*, by Von Stromer. Of this only a part of the lower jaw, the humerus and a caudal vertebra, are known. The upward turn of the superior margin of the jaw posteriorly, and the form of the humerus—particularly the quite good articular facets,—appear to me to indicate that this is a zeuglodont, with no very strong leaning toward *Squalodon*. If this be conceded, there is no way at present in which to connect the Cetacea with any group of land mammals.

I would point out in this connection that while *Microzeuglodon* is from the Eocene and is of small size, and *Squalodon* is from the

[2] *Mém. Acad. Imp. Sci. St. Petersburg*, VII^e^ Série, XX, 1873, p. vii.

[3] *L. c.*, p. 34.

[4] *Proc. Zool. Soc. London*, 1892, p. 558, pl. 36.

Miocene and Pliocene and is of comparatively large size, there is an American form of squalodont which is either from the Oligocene or Lower Miocene, and is of small size.

This is the genus *Agorophius.* It is based on a skull from South Carolina. It has serrate teeth like *Squalodon,* but what is especially remarkable, the parietals occupy a long area on the top of the skull, while in *Squalodon* and existing cetaceans the frontals and occipital come together at the vertex so as to entirely, or almost entirely, exclude the parietals.[5] The very remarkable conformation of *Agorophius* led Van Beneden and Gervais, and also Cope, to suspect that it might possibly be the progenitor of the whalebone whales. I do not think this is likely, but *Agorophius* appears to indicate that *Squalodon* may have, and probably did, originate from forms very unlike *Zeuglodon.*

It might be supposed that the whole argument concerning the derivation of the Cetacea from the zeuglodonts was negatived by the occurrence of various characteristic forms of Cetacea in the Eocene and even earlier formations, and hence contemporaneously with, or earlier than, *Zeuglodon.* In all such cases, however, so far as I have traced them, the forms reported are really from the Miocene. A notable case is that of the various important forms from Chubut, Patagonia, described by Lydekker in 1893. These include such genera as *Scaldicetus* and *Paracetus,* which certainly occur in the Miocene of North America and Europe, and, indeed, I understand the deposits at Chubut to be assigned at present without dispute to the Miocene.

The matter of the history and development of *Squalodon* is especially important, as Professor Abel derives four families of cetaceans from the squalodonts, namely, Physeteridæ, Ziphiidæ, Eurinodelphidæ and Acrodelphidæ (or Iniidæ), and one of them—the Physeteridæ—directly from *Squalodon* itself. The main argument in the latter case is that the teeth of some species of *Scaldicetus* (or *Physodon*)—an intermediate genus—have a ridge on the crown. This seems an unimportant character relatively, and does not balance the difficulty of deriving the extremely concave skull of *Physeter* from the extremely flat skull of *Squalodon.*

[5] See True, "Remarks on the Type of the Fossil Cetacean *Agorophius pygmæus* (Müller)," Smithsonian Publ., No. 1694, 1907, with 1 plate.

I think that we shall in the end come to agree with the opinion expressed many years ago by Dr. Theo. Gill,[6] that the origin of the Cetacea dates much further back than is generally believed, and that the forms above mentioned are sideshoots from a stem reaching into a much more remote past.

However it may be as to the origin of the families mentioned, Professor Abel is correct, I believe, in following the course of Gray[7] and Gill[8] in separating the sperm whales and the beaked whales into two families, the Physeteridæ and the Ziphiidæ. Abel's line of development for *Physeter* through *Scaldicetus, Physeterula, Prophyseter* and *Placoziphius* seems excellent, except that it ignores *Hypocetus* Lydek. (or *Diaphorocetus* Amegh.) of North and South America, which is certainly an ancestor of *Physeter* or *Kogia,* and probably the former.

The family Eurinodelphidæ of Abel is quite certainly distinct. While obviously allied to the Ziphiidæ, *Eurinodelphis* has distinctive characters of its own, such as the small pterygoids, very long toothless premaxillæ, a delphinoid prenarial region, etc. I succeeded in discovering a skull of this genus in the Miocene of Maryland last year and thus introducing the family into the American fauna.

Abel's family Acrodelphidæ, which, as Professor Eastman recently pointed out, should be called Iniidæ[9], while not entirely new, is a very interesting assemblage. It comprises the following subfamilies and genera:

Family Iniidæ Gill (Acrodelphidæ Abel).

Argyrocetinæ ..... { *Argyrocetus.* *Cyrtodelphis.* *Pontivaga.* *Ischyorhynchus.* *Champsodelphis.*

Acrodelphinæ ..... { *Acrodelphis.* *Heterodelphis.*

[6] *Amer. Nat.,* 7, 1873, p. 2.
[7] Cat. Seals and Whales Brit. Mus., 2d ed., 1866, p. 326.
[8] Smithsonian Misc. Coll., 11, 1872, p. 15.
[9] *Bull. Mus. Comp. Zool.,* 51, 1907, p. 86.

Iniinæ ..... { *Inia.* *Pontistes.* *Stenodelphis.*

Beluginæ ..... { *Beluga.* *Monodon.*

The partial breaking up of the currently-accepted families Platanistidæ and Delphinidæ here shown is quite radical. Usually *Platanista, Inia* and *Stenodelphis* (the so-called "river-dolphins") are united to form the family Platanistidæ, but Professor Abel leaves only the genus *Platanista* in that family. The limits of the family have always been uncertain, and Sir Wm. Flower, though accepting it provisionally in its usual form, remarked: "There are three distinct genera, which might almost be made the types of families, but it is probably more convenient to keep them together, only regarding them as representing three subfamilies."[10]

Stenodelphis, although having separate cervicals and broad lumbar diapophyses like *Inia,* has involuted pterygoids, ossified sternal ribs, and the articulations of the ordinary ribs with the vertebræ as in Delphinidæ. Associated with it is the fossil genus *Pontistes* of South America, which resembles *Stenodelphis* very closely, but is larger. The prenarial region in these genera, as well as the form and position of the nasals and the form of the zygomatic processes, recall *Phocæna* and also *Inia,* but I have been unable to satisfy myself of the importance of these resemblances.

The most radical feature of Professor Abel's classification is the removal of the white whale and narwhal (*Delphinapterus* and *Monodon*) from the Delphinidæ to the Iniidæ, although it is true that these forms had previously been considered as constituting a separate subfamily of the Delphinidæ by Gill, Flower and myself. They agree with *Inia* in having no dorsal fin, a broad pectoral, and separate cervical vertebræ, and the diapophyses of the lumbars are somewhat expanded. On the other hand, the sternal ribs are ossified, the sternum is shaped as in other Delphinidæ, the ribs articulate with the vertebræ in the same manner as in that family, and the enamel of the teeth is smooth. This combination of charac-

[10] Flower and Lydekker, "Mammals Living and Extinct," 1891, p. 258.

ters recalls *Stenodelphis* rather than *Inia,* although the former has a dorsal fin.

Professor Abel's chief reason for rejecting *Delphinapterus* and *Monodon* from the Delphinidæ appears to be that the cervical vertebræ are separate. He says that on this account they cannot be derived from Delphinidæ.[11] This seems to me illogical, for it must be true that the existing Delphinidæ with extremely thin, more or less rudimentary, and anchylosed cervicals were derived from forms with well-developed, separate cervicals. Hence, one might expect to find some forms still existing in which the cervicals are distinct. I do not think that on that account alone they should be rejected from among the Delphinidæ.

In this connection, the genus *Lophocetus* from the Miocene of Maryland is of interest. This is represented by a skull and cervical vertebræ. The skull, which is long-beaked, is delphinoid in general appearance, especially in the prenarial region, but the temporal fossæ are large and the supraoccipital narrow, and shaped somewhat as in *Inia.* The teeth are lacking, but appear to have had simple cylindrical roots. The cervical vertebræ are separate. They are, however, imbedded in the matrix, so that little can be determined regarding their characters.

This genus has been associated with *Inia* in the Platanistidæ by Cope;[12] and Dr. C. R. Eastman, who has recently given a new description of it,[13] also regards it as allied to *Inia,* while Brandt and Abel have considered it closely allied to *Delphinapterus.* I am myself inclined to the latter view, although conceding that the shape of the supraoccipital is inioid. If this be accepted, we have in *Lophocetus* a Miocene delphinoid form with separate cervicals.

On account of the combination of characters presented by *Stenodelphis, Delphinapterus, Monodon* and *Lophocetus,* three courses are possible as regards their classification. They may be included in the family Iniidæ, or made the basis of a separate family Stenodelphidæ, or included in the family Delphinidæ. The latter course seems to me best at present,

[11] *Mém. Mus. Roy. Hist. Nat. Belgique,* 3, 1905.
[12] *Amer. Nat.,* 1890, pp. 606 and 615.
[13] *Bull. Mus. Comp. Zool.,* 51, 1907, p. 79.

Professor Abel has described a delphinoid form from the Upper Miocene of Antwerp—*Pithanodelphis*—in which the atlas and axis are united as in existing genera. It would appear, from this and other evidence, that the family Delphinidæ was differentiated as early as the Miocene and that both forms with separate cervicals and forms with united cervicals were then existing.

The family Saurodelphidæ of Abel comprises the single genus *Saurodelphis* Burmeister, from the banks of the Paraná River, Argentina. The geological horizon is understood to be Pliocene. Professor Abel considers that it cannot be associated at present with any group of toothed whales, but it appears probable from Burmeister's figures that the skull has a maxillary hood and other characters resembling those of *Platanista,* and the teeth are also similar in some respects, especially as regards the growth of irregular roots with age, etc. For these reasons, I think it should be assigned to the Platanistidæ, at least provisionally.

The modifications which I have proposed in the classification of the toothed whales are summed up as follows:

CETACEA.

ODONTOCETI.

+ Squalodontidæ.

Physeteridæ. ..... { Physeterinæ. / Kogiinæ.

Ziphiidæ.

+ Eurinodelphidæ.

Iniidæ. ..... { Iniinæ. / Argyrocetinæ. / Acrodelphinæ.

Delphinidæ ..... { Stenodelphinæ. / Delphinapterinæ. / Delphininæ.

Eurinodelphidæ.